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INHERITANCE IN A HEPATIC[1]

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I

THE studies here outlined were suggested by the presence in Sphaerocarpos of these advantageous conditions: (1) The presumable relative simplicity of inheritance in a haploid generation, yielding hybrid ratios that may be tested in comparatively small families, free from complications of dominance and recessiveness. (2) The opportunity for an exhaustive study of a given genotype, expressed through a clone ordinarily requiring little space but potentially unlimited in extent and duration. (3) The permanent adherence, in most species and races, of the four spores derived from each mother cell, making possible a virtually immediate determination of the genetic effects of meiosis.

[1] Address of the retiring vice-president of Section G—Botany, American Association for the Advancement of Science, Des Moines, Iowa, December 31, 1929.

While the predominant generation of Sphaerocarpos is simpler, because haploid, than the diploid organisms chiefly studied by geneticists, the phenomena of inheritance in this genus need not be expected, and do not appear, to be simpler in any other sense. The genus is probably the present climax of as long an evolutionary development as is the pea or the banana-fly. Sexual differentiation, so far as concerns gametes and the organs in which they are borne, is at approximately the highest point reached by any plant. Dioecism and sexual dimorphism are fixed and constant. The chromosome complex is comparable with that of any "higher" organism. Allosomes, in differentiation and in the correlation between their distribution and the appearance of sexual characters, resemble those of many Metazoa. The frequency of mutation is comparable with that elsewhere observed. In the longest series of experiments yet summarized,

1,273 gametophytes of known ancestry were determined as to sex and vegetative character. Of these, five—about 0.4 per cent.—were apparent mutants. Two were marked variants never before seen.

II

It is possible in plants of various orders to study the characters of gametophytes developed from spores recognizably derived from the same mother cell. Long ago Strasburger pointed out the possibilities offered by Elodea, whose pollen grains remain adherent in tetrads. An analysis of this nature has been made by Wettstein of the distribution of certain characters in moss hybrids, and a considerable number of studies of Basidiomycetes have dealt with what is in effect the intra-tetrad distribution of factors affecting syngamy.

In the course of the work with *Sphaerocarpos Donnellii* Aust., extensive attempts have been made to apply this method of intra-tetrad analysis. The far-from-simple behavior of certain characters later mentioned has interfered with the success of many of these attempts. Rather satisfactory results have accrued, however, with respect to the distribution of sexual, and of at least one pair of what may be called vegetative, characters.

Thus far each clone, however long under cultivation or to whatever conditions subjected, has remained strictly male or strictly female. My observations, and those reported by others, have disclosed no change in sexual expression, no suggestion of hermaphroditism or of intersexuality. The female gametophyte is now well known to possess a large X-chromosome; the male has a very small Y-chromosome; the other seven chromosomes apparently correspond in size in the two sexes. The X- and Y-chromosomes are separated in meiosis; two spores of a tetrad receive each an X-chromosome and develop, if they germinate, into female gametophytes; two receive each a Y-chromosome and develop, if they germinate, into male gametophytes. To this rule my experiments have afforded no exception.

Polyclady, fully described elsewhere, affects the structure of practically all gametophytic organs, conspicuously of the antheridial and archegonial involucres. Female polycladous plants are completely sterile. Polycladous males are fertile with nonpolycladous females. From every such mating that yielded more than two f_1 offspring, the progeny of each sex included polycladous and nonpolycladous clones. No exception, save one mutant, has been observed to the rule that, of the spores of any tetrad resulting from a mating of a nonpolycladous female and a polycladous male, two are genetically polycladous and two nonpolycladous. Thus, a character-complex affecting the whole plant, far transcending generic lines as these are understood among bryophytes, behaves in a strictly unitary fashion.

If the two character-pairs concerned in such a mating—namely, maleness-femaleness and polyclady-nonpolyclady—were inherited independently, the f_1 generation would consist of equal numbers of clones of four types: female nonpolycladous, female polycladous, male nonpolycladous and male polycladous. That these types do not appear in approximately equal numbers was early apparent. It is now possible at least partially to explain these deviations from a simple expectation.

The genetic constitution of all the spores of a tetrad is established if all, by germination, give rise to clones that live long enough for their characters to be determined. If three spores thus produce clones, then, on the assumption justified by experience that two are genetically female and two male, two nonpolycladous and two polycladous, the genotype of the ungerminated spores is established. If two spores germinate, and the resultant clones are alike either in sex or in vegetative character, on the same assumption the genotypes of the two ungerminated spores are known. Of thirteen f_1 families whose fathers were polycladous, seven, as shown in Table I, have yielded material for an analysis in terms of tetrads. In the table only those tetrads are included which, as just indicated, were decisive.

TABLE I

GENETIC TYPES OF SPORE TETRADS (DECISIVE CASES ONLY) AMONG OFFSPRING OF NONPOLYCLADOUS × POLYCLADOUS

(p. = polycladous; np. = nonpolycladous)

Mating	I	II	III	Totals
	♀ 2np., ♂ 2p.	♀ 2p., ♂ 2np.	♀ 1np., 1p. ♂ 1np., 1p.	
14	—	1	—	1
28	2	1	—	3
29	5	2	1	8
31	12	7	3	22
32	13	6	5	24
33	7	1	2	10
40	—	1	1	2
Totals	39	19	12	70

Three classes of tetrads appear. Classes I and II are "two-type" tetrads; that is, both spores transmitting the same sexual tendency carry also the basis of the same vegetative character. Class III is composed of "four-type" tetrads, the spores of each representing the four possible combinations of sexual and vegetative characters.

The two classes of two-type tetrads differ in frequency of occurrence. Those of class I are about twice as numerous as those of class II, and a like difference holds for each family of any considerable size. The more numerous class is that whose spores represent the character-combinations of the respective parents; the smaller class, that whose spores represent new combinations of characters. A linkage between sex and polyclady is clearly indicated.

The familiar conception of sex-linkage is that of a character whose differential factor is borne on a sex chromosome. To apply that conception here, it would be necessary to assume a factor-pair borne on the X- and Y-chromosomes, and a crossing over during meiosis in about one third of the spore mother cells between each of the factors in question and some sex-determining factor. Apart from the questionable legitimacy of postulating a single pair of effective sex-determining factors (or even of factor-complexes behaving as units), the great difference in size between the X- and the Y-chromosome makes it extremely difficult to imagine a conventional crossing over between them.

A more probable explanation of the present facts seems to be that two chromosome pairs are concerned; one the X-Y pair affecting sex, the other a pair carrying the genetic bases for polyclady and its absence. The two chromosomes, one of either pair, derived from each parent must then tend to remain together and to pass to the same pole in each reduction division; and this tendency must be so strong that the chromosomes from the same parent do thus remain together twice as often as they are separated. Evidence is available in several angiosperms of an end-to-end attachment of chromosomes during meiosis which may lead to exactly such a behavior of chromosomes from the same parent as is here postulated. Such attachments are best known in Oenothera, but are not peculiar to that genus.

The occurrence of a minority of four-type tetrads bears directly upon the nature of meiosis. Cytological studies indicate that, in general, it is in the first reduction (heterotypic) division that the chromosomes of each pair are separated—one of the pair being in origin maternal, the other paternal. Evidence from plants is virtually unanimous on this point. In some insects, however, certain chromosome pairs, the sex chromosomes or others, are or may be separated in the second (homoeotypic) division. Now, if segregation occurs exclusively in the first division, the four nuclei ultimately formed (spore nuclei in the present case) must be, genetically, of but two sorts. That four-type tetrads occur implies that some segregation, of whole chromosomes or of chromosome parts, takes place in the second division.

Is it possible that in Sphaerocarpos *all* segregation occurs in the second division? A scheme for such segregation can be devised that will account for the production of two-type and four-type tetrads in the proportions here observed. But such a scheme involves rather far-fetched assumptions. On the simplest postulate of chance segregation in the second division only, equal numbers of two-type and of four-type tetrads should appear. *A priori* considerations then make segregation exclusively in the second division improbable. The argument is supported by the cytological evidence that segregation *appears* in virtually all plants studied to occur in the first division, and is made conclusive by the fact that in Sphaerocarpos the X-Y pair has been shown to separate in the first division—although the limited observations do not exclude the possibility of an occasional postponement of this separation to the second division.

It follows that some segregation is possible in the course of *both* divisions. Interestingly, a similar conclusion is reached by Dodge in his study of inheritance through ascospores in *Neurospora sitophila*. In that, as in the present case, two possibilities suggest themselves: either that some or most of the chromosome pairs are separated in the first division, at least one pair, however, separating regularly or occasionally in the second; or that all pairs separate in the first division, their separation being preceded by a crossing over in at least one pair of autosomes in the "four-strand" stage.

From the proportions shown by Table I for the respective classes of tetrads, it is easy to calculate the proportion in which clones of the four possible types would be expected to appear in the f_1 generation. The calculation gives: 9 ♀ nonpolycladous: 5 ♀ polycladous: 5 ♂ nonpolycladous: 9 ♂ polycladous. With this ratio may be compared the total numbers of clones of the various types obtained in f_1 progenies, as shown in Table II. Since knowledge of tetrad relationships is not essential here, this table includes much more material than did Table I.

The numbers of female types shown in Table II agree closely with the calculated proportions (266:148). The numbers for males depart rather widely from calculation, which, based upon numbers of males only, would give 118:213. As to this discrepancy, it is to be said that, since males succumb to unfavorable conditions more quickly than females, the major portion of the clones that died before their character could be satisfactorily determined were doubtless male. The females therefore present a more accurate picture of the genetic make-up of the population. Apparently it is necessary to assume in addi-

tion some difference in viability between the two types of genetically male spores.

TABLE II

RESULTS OF MATINGS OF NONPOLYCLADOUS × POLYCLADOUS; TOTAL OFFSPRING

p. = polycladous; np. = nonpolycladous)

Mating	♀ np.	♀ p.	♂ np.	♂ p.	p. (sex?)	Undet.
12	—	—	1	—	—	—
13	46	28	28	21	3	11
14	17	9	9	7	—	11
15	34	30	29	13	3	28
27	19	14	14	12	—	4
28	6	6	6	4	—	—
29	5	5	9	8	—	2
30	—	—	—	2	—	—
31	31	11	22	20	—	3
32	39	24	23	25	—	12
33	22	2	8	12	5	9
34	34	20	23	33	—	3
40	6	6	1	1	—	1
Totals	259	155	173	158	11	84

If another character-pair were concerned, ratios would evidently become more complex. Some matings have involved "tuftedness" in addition to sex and polyclady. The tufted character, as will appear, is variable, and results obtained with it must be stated cautiously. An additional difficulty is introduced by the fact that polyclady masks tuftedness, so that a polycladous tufted clone can not be distinguished by its appearance from a polycladous nontufted clone of the same sex. However, a few reasonably definite results may be cited. If a mating is made in which the father is polycladous and either father or mother is genetically tufted, then, apart from certain apparently possible but rare distributions of the tufted character, four classes of two-type tetrads and eight classes of four-type tetrads may be expected. A limited number of such matings have apparently yielded all the expected two-type classes and at least four of the eight expected four-type classes. Since the numbers of classifiable tetrads are yet low, and since the four-type classes must appear in but small proportions, it is probable that a mating of the sort in question would, in a numerous progeny, yield all the expected tetrad classes. Of those classes that have appeared, the small numbers do not justify a conclusion as to proportions, save that, as before, the two-type classes whose spores carry the parental combinations of genetic bases for polyclady or its reverse and sex are larger than those whose spores carry new combinations of these bases.

The results here given suffice to indicate that the intra-tetrad analysis of characters furnishes a direct method of attack upon the phenomena of Mendelian segregation, making possible, for example, a more accurate explanation than otherwise of crossing-over ratios; and also that it throws some light upon details of chromosome behavior that still elude cytological observation.

III

In consequence of the great difference in size between X- and Y-chromosomes, the female gametophyte of Sphaerocarpos possesses, cell for cell, a greater volume of chromosome substance than does the male. The advantage of the female in this respect is conservatively estimated at 50 per cent. Since the mother contributes a so much greater bulk of hereditary substance to the sporophytic progeny, it might be expected *a priori* that the constitution of a sporophyte would betray more evidence of maternal than of paternal influence.

An instance of preponderant maternal influence in the sporophyte appears in the inheritance of the tendency for spores to adhere, or not to adhere, as the case may be, permanently in tetrads. All the spores produced by any sporophyte are adherent or separate according as the mother of that sporophyte carried the one tendency or the other, and quite independently of the genetic constitution of the father. The tendency in question is sex-linked in transmission, although it appears in an asexual generation. Its inheritance has been tentatively explained by a factor carried on the X-chromosome and hence transmitted only through females.

A behavior perhaps closely comparable is observed in certain interspecific hybrids. Most attempts at crossing *S. Donnellii* and *S. texanus* Aust. have failed. But numerous hybrid sporophytes have been obtained from female plants of a strain of *S. texanus* from Arkansas mated with male plants of *S. Donnellii*, and a very few from a similar cross involving plants of *S. texanus* from Mississippi. The reciprocal cross has not yet succeeded. The most readily recognized distinctions between the two species are in the form and sculpturing of the spore and tetrad walls. In these respects all the spores and tetrads produced by the hybrid sporophytes resemble those of the maternal species. Such spores have thus far refused to germinate; but so far as the results go, the inheritance of spore-form and -structure follows the same rule as does that of spore-adherence or -separation. It may or may not be of wider significance that the only sporophytic characters whose inheritance it has yet been possible to study are determined by the genetic constitution of the mother.

The possible transmission of a preponderant maternal influence through the sporophyte to f_1 gametophytes is a less simple matter. The X-chromosome passes, apparently intact, to the female gametophytes; the Y-chromosome to the male gametophytes. With respect to the autosomes, the female and male gametophytes are apparently alike; apart, therefore, from differences between the sexes correlated with the presence of an X- or a Y-chromosome, no preponderant influence, either maternal or paternal, would be looked for in the f_1 gametophytes. Nevertheless, with respect to two characters something like a predominant maternal influence seems to be demonstrated in this generation.

One character is that designated "semisterile." Several semisterile male clones originated in plants of a single group, probably of common origin. They produce few antheridia. The semisterile character has remained constant through vegetative multiplication for more than eight years. A semisterile male mated with a tufted female produces typical and tufted offspring of both sexes, the typical (nontufted) tendency in this case being apparently inherited from the semisterile male ancestor. But no semisterile male, mated with a typical, tufted or appendiculate female, has ever produced semisterile offspring. The obvious explanation, that spores are formed carrying the semisterile tendency but unable to germinate, seems to be negatived by the fact that in numerous cases three, and in fewer instances all four, spores of a tetrad have germinated. It appears thus far, then, that the semisterile tendency, though constant so far as vegetative multiplication is concerned, can not be transmitted by a male parent through gametes and spores.

The second character, "cupulate," appeared in a male mutant. A cupulate male bears small crowded antheridia subtended by involucres which are predominantly saucer- or cup-shaped. This character has remained constant for five years through vegetative multiplication. The clone has proved fertile in two matings. In the one f_1 family obtained, no cupulate offspring appeared. So far as present results show, the cupulate tendency also is not inherited from the male parent through gametes and spores.

Thus phenomena suggesting the exercise of a predominant effect by maternal substances are found in both sporophyte and gametophyte. Those observed in the sporophyte may be explained by influences emanating from the X-chromosome; those noted in the gametophyte are not thus readily explainable, unless it can be imagined that factors borne on the X-chromosome may, by crossing over or by other means, be transferred to male progeny.

However, such a failure of paternal characters to reappear in f_1 gametophytes has much in common with certain instances of matrocliny in angiosperms which now tend to be explained in terms of cytoplasmic inheritance. A similar conception is nowhere more applicable than in a bryophyte, the cytoplasm of whose zygote is almost exclusively maternal.

IV

A somewhat similar failure of paternal tendencies to manifest themselves in the offspring may assist in explaining the puzzling nature of the inheritance of "tuftedness." This character distinguishes certain clones of *S. Donnellii,* the frequency of whose appearance in my original cultures suggests that tufted races are not uncommon in nature.

Tufted clones of either sex are marked by peculiarities in manner of growth and to some extent in vegetative structure, but most conspicuously by aberrancies in the forms of archegonial and antheridial involucres. A tufted clone produces tufted, typical and transitional branches, each of which may become an independent plant. The appearance of such a variety of branches is comparable with the variation that occurs among the branches of a variegated clone of Coleus, or with the less frequent appearance of bud sports in Citrus. In Sphaerocarpos the sporting process is reversible; a clone originating in a tufted branch gives rise sooner or later to typical branches, and a strictly typical branch from a tufted clone produces a clone that in time includes tufted branches. On the other hand, strictly typical *clones* exist which never produce tufted branches. Tufted clones and typical clones, then, differ genetically.

Their genetic difference is shown in another way. The f_1 progeny of a mating between a typical and a tufted clone includes both typical and tufted gametophytes of each sex. A mating between two typical clones gives rise to a gametophytic progeny exclusively typical—rare mutants being left out of consideration in each instance.

As between clones, differences in degree of expression of the tufted character are often conspicuous. While any clone varies from time to time in its degree of tuftedness, nevertheless, some clones have always during a term of years displayed a very large proportion of tufted branches—running, say, from two thirds to nine tenths or more. Others, growing during the same period in similar soil, in the same Wardian case, and subject, so far as possible, to precisely similar conditions, have shown only once or twice in a long life a few, possibly only one or two, distinctly tufted branches. Facts like these indicate that fixed hereditary differences, expressing themselves in differences in degree of tuftedness, exist between tufted clones.

The character of a clone may be indicated by the percentage which the total number of observed tufted branches bears to the total number of branches constituting the clone at the times at which a series of observations were made. Since this figure is based upon estimates and since the character of a clone varies, presumably in response to environmental changes, the absolute percentages calculated are not to be taken too seriously. They do, however, give a rough measure of inherent differences. Percentages so obtained range from a small fraction of one per cent. to as high as 98 per cent. One hundred per cent. tuftedness is never found; no tufted clone, living for a considerable period, has failed to produce at least an occasional typical branch. Tufted clones, then, constitute a series of an indefinite number of terms, ranging from almost complete tuftedness down to transitional types in which the distinction between tuftedness and nontuftedness is necessarily subject to error.

For some years experiments have been under way to test the effect of selection within tufted clones. In one series, a highly aberrant branch of each tufted clone was selected. This branch, isolated, served as the starting-point of a new clone. After the new clone had grown for some months, one of its most markedly tufted branches was similarly isolated, and the process has been repeated in various lines from eight to twelve times. In a parallel series, beginning with the same parental clones, a selection was similarly made in each instance of a strictly typical branch.

The result of these experiments was the production of clones which, growing for say six months, produced in the one series an increased proportion of tufted branches, and in the other series, an increased proportion of typical branches. Vegetative selection within a clone was to this extent effective. But when selection was suspended for from fourteen to seventeen months, the clones, representing the seventh or eighth selection in each line, growing and being transplanted from time to time, both those of high and those of low tuftedness reverted toward the average condition of the original parental clone. Intraclonal selection had not resulted in a lasting genetic change.

The question, however, arises how the apparent *temporary* change in character could be effected by selection. With this temporary effect of selection may be correlated the observation that a large tufted clone commonly includes patches, some of distinctly tufted, others of strictly typical, branches. Doubtless the expression of the tufted character is conditioned or affected by the environment. But in a three-inch pot of well-mixed soil, in all parts equally illuminated and equally moistened, it is difficult to explain the patchwork condition just described by localized differences in environment alone. Both the tendency to a patchwork arrangement and the temporary effects of selection indicate that a tufted branch tends more strongly to produce new tufted than new typical branches, and that a typical branch within the same clone similarly tends more strongly to produce new typical branches. To this behavior, also, analogies are to be found among angiosperms; see, for example, Darwin's classical discussion of bud variations, and Stout's study of variegated clones of Coleus.

It would be rash at present to attempt to formulate the difference in cellular constitution which, existing between a tufted and a typical branch, must supply the basis for a temporary difference in their (vegetative) reproductive behavior. Whatever the difference, it is superposed upon a fundamental and persistent genetic identity. The temporary difference may *possibly* be referable to some feature of cytoplasmic structure, which ultimately yields to the regulative action of the chromosomes.

It has been noted that genetic differences of a permanent nature seem to exist among tufted clones. To test the reality of such differences, matings have been made involving nontufted, little-tufted (1 to 12 per cent.) and much-tufted (55 to 93 per cent.) clones. The results of these matings, as determined by the characters of the f_1 gametophytes, are here presented in much-abridged form. Fuller details are embodied in a report now in press.

The possibility of error in the determination of genetically slightly tufted clones has already been alluded to. The danger of such error is greater in the classification of male than of female clones, because a larger proportion of males succumb comparatively early. With the qualification, therefore, that some errors of this type have undoubtedly occurred, the following results are presented.

Generally speaking, the more highly tufted parents produce on the average the more highly tufted offspring, less tufted parents less tufted offspring. While this is true in terms of averages, the individual clones of any family show a wide range in their proportion of tuftedness. One family (24) may be selected as illustrative. The average tuftedness of the mother was 6 per cent.; of the father, 12 per cent. The f_1 clones whose character was determined with some confidence included 37 nontufted and 38 tufted females, 14 nontufted and 12 tufted males. The degree of tuftedness among f_1 females ranged from less than one to 39 per cent.; among f_1 males, from 15 to 90 per cent. The average tuftedness of all tufted females was 11 per cent.—about twice that of the mother and approximately equaling that of the father. The average tuftedness of all tufted males

was 44 per cent.—seven times that of the mother and nearly four times that of the father. The average tuftedness of all females, including nontufted, was 6 per cent.; that of all males, 20 per cent. The occurrence and distribution of so varied conditions within a single family fit the conception previously formulated of the existence of genetic differences between different tufted clones, leading to complexity in the method of inheritance of tuftedness.

The apparent intra-tetrad distribution of characters among the offspring of three types of matings involving tuftedness is shown in Table III. In this table was included every tetrad of which even a single spore gave rise to a clone whose character was determined. Thus, in the first column of the table tetrads are included which gave rise to but one or two nontufted females or to one or two tufted males.

TABLE III

APPARENT INTRA-TETRAD DISTRIBUTION OF OFFSPRING OF MATINGS INVOLVING TUFTEDNESS

(t. = tufted; nt. = nontufted)

Parents	2 ♀ nt., 2 ♂ t.	2 ♀ t., 2 ♂ nt.	Other tetrad groupings
Nont. × Much-t. (4 matings)	30	32	28
Little-t. × Little-t. (2 matings)	36	35	
Little-t. × Much-t. (1 mating)	6	8	1

Most of those tetrads produced by matings of nontufted × much-tufted that are classed under "other tetrad groupings" would fall into either the first or the second class in Table III if certain clones determined as nontufted really were genetically tufted. It is probable that in some proportion of these tetrads such a correction should be made. The class of "other tetrad groupings" includes, however, some tetrads three or all of whose spores bore the tufted tendency; possibly some tetrads three or all of whose spores were genetically nontufted, and apparently a few four-type tetrads.

The one instance among "other tetrad groupings" derived from a mating of little-tufted × much-tufted was apparently a four-type tetrad.

If tuftedness were inherited as though determined by a single factor, a mating of nontufted × tufted would be expected to yield tetrads of the two classes shown to be present in largest numbers, plus possibly some four-type tetrads. On the same basis, a mating of tufted × tufted would be expected to yield only tufted offspring. The odd fact indicated by Table III is that the matings therein included of tufted × tufted gave results agreeing more closely with the expectation, on a simple genetic basis, from matings of nontufted × tufted than did the results of the latter matings themselves.

In contrast, families derived from two much-tufted parents agreed closely with expectation. The offspring of five matings of this type were all, or almost all, tufted. Apparent exceptions were one female, a polycladous mutant in which tuftedness may have been masked by polyclady; and three apparently nontufted males, any or all of which, for reasons already given, may have been genetically tufted. The remaining 84 females and 42 males were unquestionably tufted.

These results, taken together, confirm the conclusion derived from other evidence that tufted clones may differ genetically. Stated in factorial terms, this implies that tuftedness is or may be due to several distinct factors, some or all of which are cumulative in their effects. But such multiple factors alone will not account for the intra-tetrad distribution of characters in the various types of matings described. An explanation offered in another place, postulating in addition complementary factors and certain linkage relations, fits fairly well the facts now available. This hypothesis need not be detailed here. It involves several assumptions, unfounded save as they were made to fit a complicated state of facts. The hypothesis may be somewhat simplified if it can be supposed that, as in the case of the semisterile and cupulate characters already discussed, certain factors contributed by a male parent may disappear or be inactivated in the offspring. It is true that not all paternal factors basic to tuftedness can be assumed always to be inactivated, for tuftedness has appeared among the progeny of nontufted mothers and tufted fathers. The suggestion of a maternal influence upon the expression of this character thus remains a mere suggestion, justified, however, by the observed fact that among the relatively few characters, sporophytic and gametophytic, yet experimented with, a surprising proportion, as pointed out earlier, evidence some preponderant influence on the part of the mother.

Possibly it may be permissible here to note that the behavior in inheritance of tuftedness does not, in itself, suggest the dependence of that character upon any number of specific, separable factors. The picture presented by the facts now known is that of a continuous series of genetic states ranging from the complete absence of tuftedness to its almost, but never quite, universal distribution over a clone of indefinite extent. The question might be posed whether, in dealing with a character of this nature, a

factorial hypothesis can ever offer more than a purely formal explanation.

V

The results here presented, though incomplete, serve to indicate that the genetic behavior of different organisms may in important respects be very diverse, even though in other respects far-reaching similarities appear between forms so far apart phylogenetically as bryophytes, flies, angiosperms and mammals. It follows that a comprehension of the facts of inheritance and variation, and a recognition of the fundamental features of their mechanism, require the assemblage of information from all available sources, including adequately representative members of all plant and animal phyla.

THE ORGANIC WORLD AND THE CAUSAL PRINCIPLE[1]

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SOME thirty centuries ago a Hebrew sage summed up the outcome of his observations by declaring that "there is no new thing under the sun." In those days man's knowledge of nature was absurdly limited. A contemporary critic might perhaps have challenged the dictum, but a final decision could not then be reached. As a matter of fact, Solomon, in those words, set forth a problem which has haunted the thinking man even to this very day. We are still asking the question: Does anything really new ever appear in the universe?

Taken superficially the statement is manifestly untrue. New aggregations of atoms are constantly taking place. Every fresh geological stratum, every individual organism, whether plant or animal, every chemical compound, is a new construction. But on the other hand, science is constantly demonstrating that these apparent novelties result, one and all, from the operation of certain general principles which hold throughout the entire known universe and which seem to have held throughout all time. The problem, as I see it, is really this: Granted that the material world is constantly changing, granted that all changes proceed according to certain rigid causal principles which we call the laws of nature and that these principles hold throughout the physical universe, we must ask: Has there appeared in the course of history any *new* principle—any mode of activity which is more than a direct corollary from these universal and eternal principles of cause and effect?

The answer is still very much in doubt. Those who incline to a mechanistic view of nature generally accept Solomon's conclusion. Vitalists and teleologists take the opposite position. They assume that new forces have somehow been brought into existence with the advent of organisms and conscious beings into the world. And there has recently come into prominence another conception of things, the theory of emergent evolution, which may take either a vitalistic or a mechanistic form, but which distinctly challenges the ancient aphorism. This theory also declares emphatically that new and unpredictable properties come into being from time to time as new systems of material units are generated.

With the vitalistic conception I have never had much sympathy. It may reasonably be assumed that most of the novelties which seem to emerge in the course of evolution are not strictly novelties at all—that the laws which govern the activities of these higher complexities of matter might have been formulated in advance, as corollaries from the fundamental causal principles. This remains for future experiment and logic to settle. What I wish to point out this afternoon is that the advent of the organism and its evolution have apparently given rise to two new principles of activity in the universe—principles which, so far as I can see, are not in any way deducible from the universal laws of causation. To this extent the conception of emergent evolution seems justified.

The various laws of cause and effect, so far as they have been discovered and formulated to-day, may be summed up, I take it, under the general principle of the conservation of energy. This principle is by no means self-evident. To the casual observer every activity requires expenditure of energy; in other words, work or effort seems at first sight to involve the disappearance of a certain amount of energy. It required the most delicate experimentation to demonstrate that the energy in question is not actually lost, but is merely transformed. However, the truth of the principle of conservation seems now well established. The total effect is believed to be exactly equivalent to the sum total of the causes; in every change that occurs, the total consequents are exactly equal to the total antecedents. This principle might appropriately be termed the law of the unvarying total.

[1] Address of the retiring vice-president and chairman of Section I—Psychology, American Association for the Advancement of Science, Des Moines, December, 1929.

My objection to vitalistic and teleological theories is that they seem to assume exceptions to this general causal principle. Their line of argument, so far as I am able to analyze it, seems to imply that in the organic world certain phenomena of growth and behavior involve changes in which the total later state is more than or essentially different from the totality of its antecedents. In fact, vitalistic biology and purposive psychology seem to cast doubt on the uniformity of nature, which is perhaps more fundamental than the principle of conservation. I shall not go into this question, which is really aside from our present topic. What I wish to bring out is that one may adopt a thoroughly mechanistic view of the world, which holds that the principle of the unvarying total is without exception, and at the same time believe that certain new principles of activity operate in connection with organic life.

The first of these supplementary principles is that of natural selection. There is nothing in the operation of natural selection that involves any exception to the universal causal principle. The changes that occur in connection with the growth of the individual organism and the changes of type which mark the evolution of species may all be brought under the principle of the unvarying total. And yet something new does occur in evolution. Organisms tend to become more adapted to the conditions of their environment: they respond to a larger number of stimuli; their responsive activities tend to afford them more and better assistance in keeping alive, in maintaining their status as organic units and in utilizing the surrounding world for their own welfare. Every step in progressive evolution proceeds, I take it, strictly according to the causal principle. At the same time, this process (or fact) of increasing adaptation is something quite novel: it does not occur—it has no meaning whatever—except in connection with those peculiar groupings of molecules which we call organisms.

Natural selection means simply that the fittest organisms tend to survive. They survive because they are more adapted to the general environment than other organisms. The principle is almost tautological. But could it operate in connection with any material except the biological organism or some unitary system which possesses the essential characteristics of the biological organism?

Selective evolution requires the continual production of complex individuals which resemble one another. It requires also that these individuals shall differ to some extent from one another. These conditions are fully met in biological organisms through their specific mode of reproduction. Since the offspring bear a close resemblance to the parent stock, the same types of organs and the same modes of behavior reappear generation after generation. Since the offspring differ in certain respects from the parents, there is opportunity for selective improvement in any of these organs and modes of behavior. It would be assuming too much to say that nowhere in the universe does selective evolution occur except among organisms such as exist on the earth. Other systems which fulfil these essential conditions may have been built up in other ways and out of other chemical elements on other planets. My point is that these conditions do not exist below the organic level. The principle of selective adaptation would seem to be a brand-new mode of activity, which does not operate in simple aggregations of matter or in any of the simpler systems of chemical compounds.

Let me apologize for presenting to this audience a form of argument which smacks of the philosopher rather than the scientist. My excuse is that the doctrine of emergent evolution itself is a philosopher's view of the world and has been bolstered up chiefly by philosophical evidence. The mechanist suspects, though he may not be able to prove, that the so-called new properties which emerge in new compounds and new systems are not really novel. Given sufficient knowledge of elementary properties, I believe that the properties of the higher compounds could be predicted—in other words, that the higher laws which we formulate with respect to their activities are merely corollaries from the fundamental principle of causation. But the principle of natural selection, or selective adaptation, does not seem to be in any sense a corollary from the causal principle.

It is perhaps unnecessary to-day to emphasize the importance of selective adaptation in promoting organic evolution. Through its means the organization of matter takes an entirely new trend. Spencer's mechanistic formula for evolution may cover the progressive changes of cosmic phenomena in general, but it proves inadequate to express the full import of organic evolution or the results that attend the interplay between organisms and their environment. The evolution of special organs for nutrition, circulation, responsive activity, reproduction, defense, social communication between organisms, introduces a novel feature into the material world. This new trend, according to the mechanist, represents no exception to the general principle of causation—it involves no modification of that principle. But it does indicate the operation of an additional principle, that of natural selection. In the jargon of the logician, the principle of selective adaptation may be said to be superimposed upon the principle of the unvarying total.

Of what interest is this to us as psychologists? It seems to bear on our branch in two important ways. In the first place, selective adaptation enables us to understand the evolution of behavior. Response to stimulation is an indirect reaction. In addition to the direct and immediate physical action-and-reaction which occurs when a force is applied to a body, we observe in the case of organisms a secondary and later reaction which takes place when the given force is applied as a stimulus to a receptor organ. And we note that this indirect reaction or response tends to be adaptive—it serves in most cases to promote the organic welfare of the creature. These adaptive responses depend upon the presence in the organism of certain specific organs for reception and for motor activity, and of a conducting system for establishing proper connections between these two groups of organs. The evolution of this entire behavior mechanism depends upon natural selection. It follows, then, that the real meaning of responsive behavior is bound up in the operation of selective adaptation, so that this principle concerns us fully as much as it does the biologists.

The second point of interest to psychologists lies in the fact that the selective principle does not complete our explanatory theory. I shall try to show that a still higher principle becomes effective in the later stages of organic evolution. In the more advanced animal species behavior may be a response not merely to present stimulation but to future situations. The phenomenon of anticipation or foresight is, to all appearances, something novel, which occurs only in organisms of a certain degree of complexity. The frog that snaps at a fly, the squirrel that buries a store of nuts and later retrieves them, the man who builds and furnishes a home for a prospective bride—all these are responding in a measure to future situations. A large part of human behavior has more or less definite reference to the future, and one could cite a host of such instances in subhuman species. The fact that responses are made in anticipation of future conditions is responsible for the rise of the teleological school of psychologists, which may be regarded as the counterpart of the vitalistic school in biology.

Now just as the evolution of organisms seems to admit of explanation on the basis of mechanistic causation, so the evolution of anticipatory responses and their mode of operation seems explicable in terms of the same general causal principle. But the anticipatory type of response depends upon certain special factors and conditions which come into play only in the higher organisms. In these higher species certain organs and mechanisms have evolved which enable them to respond in a preparatory way to stimuli which have not yet been applied and to situations which are still forming. Distance reception is one of these factors. The distance receptors enable a creature to see, hear or smell an object which is not in immediate contact with his body. If the object is approaching him, he may respond to the visual, auditory or olfactory stimuli in such a way as to be prepared for the contact of the object when it finally impinges upon his body. Furthermore, if the object happens to be a nutritious substance he may respond by locomotor activity, and hasten the advent of the tactual and gustatory stimuli by moving toward it. Or if the object is something injurious he may act so as to prevent the contact stimulus altogether.

Another factor which promotes anticipatory responses is the more or less permanent registration of the effects of past experiences in the nervous system. We are still in doubt as to the physiological mechanism of this registration. But the fact itself is perfectly evident. And it has this result. When a situation occurs which resembles a situation that the animal has previously experienced, the earlier response may be carried out more quickly and more precisely than it would if the situation were new. Not only this, but in many cases of repeated behavior the motor activity which takes place is a response to a phase of the situation which followed somewhat later on the previous occasion. Classical examples of this are the burnt child who withdraws his finger before actually touching the candle flame; and the bird who, after escaping from the trap, responds to similar baited objects by flying away. The literature of conditioned reflex experimentation is filled with examples of the effects of neural registration. In almost every case the result of conditioning is an anticipatory or at least a preparatory response. And whenever we examine the process by which these preparatory responses are acquired and trace the details of their operation, we find no indication of any modification of the causal principle—no exception to the principle of the unvarying total. In every case the movements and changes that occur are just what the physicist and chemist could have predicted, if they had had knowledge of all the immediate antecedents.

The teleologist, it should be noted, ascribes the fitness of anticipatory responses to a certain peculiar characteristic of consciousness which he calls awareness or insight or realization of the situation. But in practical experience we find that this awareness or insight has no power whatever to initiate the appropriate motor response unless the requisite neural connections are already established. If a man, by taking thought, can not oscillate his own ears, how can conscious reflection enable him to perform more complicated acts unless the proper neural connections are established? Conscious foresight, taken by itself,

seems incapable of explaining anticipatory responses. The principles of causation and selection, exemplified here in the learning process, furnish the basis for an adequate explanation. The teleological theory, then, seems to violate the canon of parsimony. It multiplies causes and agencies beyond our need.

And yet if we wish to understand these anticipatory phenomena completely, we must look beyond mere causation and adaptation. There is something further to be considered. We have not fathomed the entire significance of the phenomena when we have traced the evolution of the organs and their functions. Anticipation of the future is a fact which demands recognition. It does not seem to be in any way implied in natural selection, though it obviously depends upon the operation of that principle. Anticipatory adjustment is a novel type of response which is observed in certain highly complex organisms. The full meaning of their behavior is realized by the observer only if he recognizes that certain of their activities are more than mere responses to present stimulation.

Recently a tornado was reported in the Caribbean Sea moving in the direction of Florida. Preparations were made at once to prevent loss of life and minimize the damage to property. Ships altered their course. Buildings were shored up. Dwellers in the Everglades were transferred to more elevated ground. All these human activities were in response to what stimuli? In a measure they were reactions to present verbal stimuli—telegrams, storm signals, newspaper bulletins, radio messages, individual warnings by word of mouth. I have no doubt but that if a superscientist were to trace the cause-and-effect relations of this series of responses in the case of any person involved, he would find that the fundamental causal principle accounted fully for that person's activity. But this causal explanation does not exhaust the meaning of the behavior. The activity of some thousands of individuals in this instance had reference to a certain future situation as well as to the present. As a matter of fact, in cases like this the immediate antecedents (the verbal stimuli) may be regarded as merely incidental—the responses were primarily to stimuli which were yet to come.

A large part of human behavior is of this anticipatory type. In the majority of cases it may properly be called purposive, since the responsive activity is generally preceded by some conscious picture or imaging of the prospective situation. But we need not regard conscious purpose as a distinct type of preparatory response. It is merely more complex and more adaptive. In simpler instances, especially among subhuman species, the same type of response may occur without any prevision; the present stimulus serves to set off the response, and yet the response is obviously with reference to some future situation. The action of a dog in burying a bone is an example of this, or the act of a baseball fielder in raising his hands to catch a high fly. Either of these responses may occur without any conscious foresight. The conscious representation of the coming situation seems to involve merely the operation of a more highly organized neural adjustment, which is of the same anticipatory type as automatic preparatory adjustments. The significance of all such acts, whether consciously purposive or not, lies in the fact that they are preparatory responses. Are we not justified, then, in regarding anticipatory adjustment as a new principle of activity, which appears at a certain stage of organic evolution and supplements the principle of selective adaptation?

And now let me explain my reasons for discussing this topic. I have no idea of expounding a cosmic theory or a philosophic system. But I do feel that the theoretical implications of the evolutionary process fully deserve the attention they have received. The theory of vitalism in biology and the teleological or hormic theory in psychology are attempts to provide a suitable explanation of the progress of organic evolution. The more recent theory of emergent evolution is an attempt in the same direction. It is reasonable to ask whether any or all of these views are correct interpretations of the course of evolution. Since these views all hinge directly upon one's interpretation of the causal process, the problem of the causal relation must first be considered. Until recently the principle of conservation was by no means established. Transformations and inconsequences which to-day we would call magical were conceived as possible and were assumed as actually occurring. These magical interpretations of the causal process have now been abandoned by thinking men. The principle of the unvarying total is accepted as a general characteristic of the changes that take place in physical and chemical groupings.

The status of organic phenomena is not so well established. Vital force and purposive striving are accepted to-day by a number of biologists and psychologists in good standing. These views can be refuted only if we are able to show that the course of evolution can be fully explained in terms of mechanistic processes. Nor can they properly be characterized as magical or mystical unless we are able to demonstrate that a non-mechanistic explanation involves some exception to the causal principle of the unvarying total. I see no clear evidence at present that either of these theories involves magical or mystical assumptions. But I do believe we have considerable evidence for a thoroughly mechanistic interpretation of life and mind.

The process of natural selection seems able to ac-

count in a purely mechanistic way for the progressive trend of organic evolution toward greater adaptiveness of structure and function. We need not assume that a new type of energy, a vital force or entelechy, appears in the universe when organisms come into existence. The age-long causal principles still hold in the organic realm. But we may properly concede this much to the vitalists—they are right in suggesting that we must seek a new and broader view-point when we come to deal with organic phenomena. The organism may well be regarded as a new kind of system, whose distinctive characteristic is an ability to maintain its organic unity through dynamic interplay with the environment. The inherited similarity among organisms, and their variability, taken together, give rise to natural selection. And the result of the selective process is to improve the relations between the organism and its environment. Natural selection therefore should be considered not merely as a process, but as bringing about a very definite end-result: adaptation. The process of selective adaptation, then, should be recognized as a new factor which appears in connection with organic life. It is in no sense an exception to the causal principle of the unvarying total, but it does supplement this principle. It would appear impossible to have predicted in preorganic ages, from any knowledge of causal principles, the extraordinary variety and complexity of the organisms that have actually arisen, their manifold activities and tenacity of existence, or their numerical abundance. The principle of selective adaptation seems capable of accounting for all these facts. It explains the course of organic evolution without assuming the advent of a new kind of energy to operate these changes.

The same line of argument may be applied to the teleological psychology. This theory is useful in suggesting that a new and broader view-point is required when we come to deal with the phenomena of anticipatory behavior and prevision. But the assumption of a new kind of force to account for these activities seems needless and redundant. The universal causal principle and the principle of selective adaptation seem sufficient to explain the rise of preparatory responses and conscious purpose. The processes by which organic beings act with reference to future stimuli and coming situations may be described in purely mechanistic terms. But the significance of these phenomena must be sought in something that lies beyond these fundamental principles: their real meaning is summed up in the principle of anticipatory adjustment. There is no novelty involved in this principle, except that we have adopted a broader view-point.

And now we have reached the point where we can appraise the truth and error in the doctrine of emergent evolution. Many, if not all, of the so-called emerging properties which appear in the higher aggregations of matter could be predicted, I believe, from an intimate knowledge of physical principles. They are not really novel. They emerge only as new groupings or systems come into being. But we do find two new kinds of process developing at certain stages of evolution on the earth. With the advent of organic life the trend of progress is determined by the principle of *selective adaptation*, which seems to be a distinct novelty and not a mere corollary from the general principles of physics. At a later stage the responsive behavior of certain organisms to forces and situations in the environment is determined in part by a second novel principle—that of *anticipatory adjustment*—which assumes enormous importance in human life. These two principles, based upon the fundamental causal principle of the *unvarying total*, form a hierarchy. I believe they are needed to account fully for the organic stages of evolutionary progress. At least they lend new significance to this unique development. If the doctrine of emergent evolution means the recognition of general principles such as these, I believe it can be defended. If it means that new properties appear with the formation of new aggregations of matter, it is either a truism or it is open to grave question as implying an exception to the general uniformity of nature.

SCIENTIFIC EVENTS

THE WORLD PRODUCTION OF GOLD

SINCE the discovery of America, world production of gold has only slightly exceeded a billion ounces, approximately 1,003,500,000 ounces being indicated by study of available records, according to an economic review of gold production in the period 1493–1927, recently concluded by Robert A. Ridgway and the staff of the common metals division of the U. S. Bureau of Mines. Scattered production, of which there is no record, would probably add no more than one per cent. to this estimated total.

Mr. Scott Turner, director of the bureau, points out that more than half the grand total of gold production for the past 435 years, or 516,273,000 ounces, was produced in the first 27 years of the present century. Of the cumulative world production of gold since 1492, 467,000,000 ounces are estimated to exist in the form of monetary stocks, while 53[illegible],563,329 ounces

epresent the balance of gold lost or absorbed in other han monetary uses.

The scarcity of gold may be illustrated by the fact hat if the entire world production since the discovery f America were cast into a cube its edges would mea- ure only 38.5 feet. It might well be questioned whether this gold, considered merely as a metal char- cterized by certain properties, could be worth the toil nd sacrifice required for its production; but as a asis of exchange, greatly facilitating the develop- ent of commerce among the peoples of the earth, its alue to civilization exceeds all reckoning.

In spite of a superficial claim often made that gold oes not pay its cost of production, it can hardly be doubted that United States gold production, amount- ng to more than $4,500,000,000 when reckoned roughly at $20 an ounce, must have been an important factor in assisting the development of other sources of national wealth. The British Empire production from the Transvaal (1901 to 1927), of more than 198,000,000 ounces; from Australasia, of more than 168,000,000 ounces; from Canada, of nearly 32,000,- 000 ounces; from India and Rhodesia, together, 32,- 000,000 ounces; and from other parts of the British Empire, of 12,000,000 ounces, amounting in all, since 1801, to 442,994,369 ounces, or about $9,000,000,000, should be reckoned an important factor in the pros- perity of that empire.

During the first half of the nineteenth century nearly three times as much gold was produced in the fifth decade as in the first; yet the production of the first decade of the next half century was almost four times that of the preceding decade and nearly twice that of the first half of the nineteenth century. A gradual decline is noted during this half century up to the last decade, which showed an increase of 94 per cent. over the preceding decade. During the last half of the nineteenth century nine times as much gold was produced as during the first half. During the first decade of the twentieth century the average an- nual production increased over 8,000,000 ounces, or 51 per cent., over the preceding decade's production. The second decade showed an increase in average an- nual production of over 2,000,000 ounces, or 12 per cent., more than during the first decade.

In the world production of gold from 1493 to 1927, North America was the largest producing continent, contributing 281,056,639 ounces, or 28 per cent., with Africa a close second, contributing 270,127,146 ounces, or 27 per cent., of the world total. Australasia pro- duced nearly 169,000,000 ounces, or 17 per cent.; South America nearly 125,000,000 ounces, or 12 per cent.; Asia 112,000,000 ounces, or 11 per cent., and Europe nearly 44,000,000 ounces, or 4 per cent.

THE ANNUAL EXHIBITION OF THE BRITISH PHYSICAL AND OPTICAL SOCIETIES

THE twentieth annual exhibition of the Physical and Optical Societies opened on January 7 at the Imperial College of Science and Technology, South Kensington.

The London *Times* gave the following description of the exhibition: There were two main sections devoted respectively to trade exhibits of electrical, optical and other physical apparatus, and to research and experiment, and smaller sections showing apparatus used for the teaching of physics in schools and universities, a group of historical exhibits and competitive exhibits made by apprentices and learners. The last of these was introduced this year for the first time. It is realized that scientific instruments are now used so extensively that the makers have had to turn to methods of mass production. No one man to-day makes a complete instrument, and there is a danger of skilled craftsmanship dying out. The new section had for its object the encouragement of craftsmanship, and the display represented the individual work of apprentices and learners employed by scientific instrument manufacturers who took part in the exhibition. The two classes included practical examples of electrical, optical, engineering and metal work, tool-making and pattern-making, and examples of designs, drawings or tracings for scientific instruments. Eight prizes were offered, and there were 50 entries.

All the leading makers of instruments and accessories contributed to the trade section, and the exhibits covered a very wide range. There were cameras and lenses, valves, microphones, thermometers, meters of many specialized types, accumulators, timing mechanisms, spectrographs, magnetic compasses, echo sounding machines, aircraft direction finders, a tobacco moisture tester, binoculars, telescopes, rain gauges, theodolites, electric furnaces and scores of other instruments and machines.

The research and experimental section was particularly attractive, though it is hoped that in future years its scope and utility will be further extended by the cooperation of fellows of the societies and others who may be able to contribute. Government institutions, such as the National Physical Laboratory, and the Royal Air Force Establishment, South Farnborough, sent exhibits. From the laboratory came a distant reading resistance thermometer outfit for cold storage work, experimental transmitting and receiving apparatus for ultra-short waves, apparatus for the measurement of the over-all performance of radio receivers and a method for indicating turbulence in airflow.

Exhibits of the research laboratories of the Gramo-

phone Company, Limited, and the Marconiphone Company, Limited, occupied a whole room. A model was shown of a photographic sound-recording system. The system comprises a glow lamp, the brilliance of which is modulated by speech currents from the studio microphone. A slit of light from this lamp, after traversing an optical system which reduces the image of the slit to the required dimensions, impinges on a reel of film and makes a record of the variable density type. Another H. M. V. exhibit showed a method of reproducing physically the conditions in record grooves of any desired type. The apparatus enables groove conditions to be magnified 400 times, and by its use the behavior of needles at various stages of wear and under any required groove conditions can be felt and examined at leisure. Among numerous exhibits from the research laboratories of the General Electric Company, Limited, was a model of a photo-electric street lighting unit, so designed that as daylight fails lamps are automatically illuminated. The apparatus is being tested practically in a street at Wembley. Another model illustrated a transmission tower testing plant for the load testing of overhead transmission of current—a topical exhibit in view of the proposals for the distribution of electricity by means of towers and cables. Other bodies represented in the section included the British Research Association for the Woollen and Worsted Industries, which has an instrument for measuring the thickness of fabrics, and the Gas Light and Coke Company, Limited.

The apparatus for the historical section of the exhibition was selected this year to illustrate experiments of historical importance on the determination of the mechanical equivalent of heat, and some points in the evolution of thermometers. The thermometers included reconstructions of the earliest known instruments for demonstrating the expansion of air by heat, the thermoscopes described by Philo of Byzantium (300 B. C.) and Hero of Alexandria. There was also a photograph of Galileo's thermoscope.

Three evening discourses were arranged in connection with the exhibition. Lord Rayleigh gave an address on "Iridescent colors in nature from the standpoint of physical optics"; Mr. S. G. Brown on "Gyro compasses for gunfire control," and Sir Ambrose Fleming on "The present and future of television."

THE FOREST SURVEY

THE U. S. Department of Agriculture has issued a statement announcing the promotion by the United States Forest Service of District Forester C. M. Granger, of the Pacific northwest district, to the position of head forest economist in charge of the nation-wide forest survey now being launched by the Forest Service.

The forest survey, authorized by the McSweeney-McNary Act of 1928, is one of the most important undertakings in the development of forestry thus far undertaken. It will be a comprehensive appraisal of existing forest supplies and conditions, growth and requirements, and of present and future trends, all of which, when properly coordinated, will constitute a fundamental and economically sound basis for determining federal, state and industrial forest policies and programs. Congress has authorized a federal contribution of $3,000,000 to the project. A small initial appropriation of $40,000 is available this year.

Success in developing the project will depend upon widespread cooperation with federal and state and industrial and other private agencies. Certain phases of the survey, such as the forest resource inventory and the study of growth, will be handled by the regional forest experiment stations under the general direction of Mr. Granger.

Because of its outstandingly complex and important forest problems, the Pacific northwest has been selected as the region for the beginning of intensive work. The Pacific northwest forest experiment station, of which T. T. Munger is director, has already commenced preliminary work. The study will be extended as rapidly as possible to other forest regions, and will eventually cover the entire United States.

Another step already taken is a canvass, in cooperation with the U. S. Bureau of the Census, of the wood requirements of the wood-using industries, as a part of the present and future wood requirements phase of the survey. Advance cooperative work also is being initiated in the Lake States region.

The agricultural appropriation bill now before the Congress carries an increase of $85,000 for the forest survey for the next fiscal year. It is planned to use $50,000 of this to expand the work in the Pacific northwest, $25,000 to initiate intensive work in the southern hardwood region and $10,000 for individual assignments.

Mr. Granger is a graduate in forestry of Michigan Agricultural College. He entered the Forest Service in 1907, serving successively as deputy supervisor, supervisor, and assistant district forester in charge of the offices of silviculture and of operation in the Rocky Mountain district. During the war he served with the Tenth Forestry Engineers, attaining the rank of major. In 1924 he became district forester of the Pacific northwest district. He will enter his new work with a wide experience in the development of silvicultural policies, management plans, fire protection plans and with a comprehensive understanding of the economic aspect of forestry.

Mr. Granger assumes his new duties immediately, with headquarters temporarily in Portland, Oregon.

The Forest Service also announces the appointment of Horace J. Andrews as senior forest economist, to become regional project leader for the Pacific northwest. Mr. Andrews is a graduate of the University of Michigan and received the M.S. degree in forestry from that institution in 1916. He was formerly employed in the U. S. Forest Service, and later engaged in private forestry work and in teaching at three forest schools. He was closely associated with the development of the Michigan land economic survey, and more recently has been in charge of the divisions of protection and lands in the Michigan department of conservation.

RADIO TALKS ON SCIENCE IN THE KITCHEN

EACH year since 1925 Mellon Institute of Industrial Research has been giving a series of radio talks, broadcast from the University of Pittsburgh Studio of the Westinghouse Electric & Manufacturing Company's Station KDKA. Each of the five series presented so far has been published in booklet form; three of these publications are really text-books of home economics and, in fact, are being widely used in domestic science courses.

In order to supplement these series of talks, the Mellon Institute has decided to schedule eight radio discourses on "Science in the Kitchen," on the Fridays of February and March, at 5:45 P. M., from the University of Pittsburgh Studio of Station KDKA. They will subsequently be published by the university. In general, the new series will relate to the selection, care and service of foods, and each talk will be given by a research specialist of the institute.

The complete program is as follows:

Date in 1930	Subject	Speaker
Feb. 7	"The Well Planned Kitchen"	Dr. Geo. D. Beal
Feb. 14	"The Intelligent Purchasing of Foods"	Dr. W. W. Duecker
Feb. 21	"The Care of Food in the Home"	Dr. E. W. Morrison
Feb. 28	"Good Proportions of Foods"	Dr. G. J. Cox
Mar. 7	"The Uses of Milk in the Home"	Dr. L. W. Bass
Mar. 14	"Good Meals by the Young Homemaker"	Mr. E. R. Harding
Mar. 21	"Food for Young Children"	Dr. R. N. Wenzel
Mar. 28	"School Lunches"	Dr. R. F. Beard

ANNUAL REPORT OF THE YALE SCHOOL OF MEDICINE

TRAINING of physicians and humanitarians, rather than doctor-technicians, is described as the prime objective of the Yale School of Medicine, in the annual report of Dean M. C. Winternitz to the president and fellows of Yale University. Such an aim is said to necessitate the study of man in his entirety, and great strides in the direction of making this possible were taken during the past year. Of first importance was the formation of the Human Welfare Group, with the Institute of Human Relations as the integrating agent, bringing into close contact various university divisions engaged in the study of individual and social conduct.

Gifts equivalent to $9,900,000 were obtained by the university during the year to meet financial requirements aggregating $15,500,000 for the stabilization of the activities of the group. Most of the gifts were designated for the work of the Institute of Human Relations. There was also included, however, the sum of $2,000,000 for erection of a clinic building and the medical and pediatrics laboratory on the grounds of the New Haven Hospital, and construction of these units is well under way.

During 1929, the Farnam Memorial Building, Lauder Hall and the Brady Memorial Laboratory, on the hospital grounds, were completed at a cost of $1,250,000, and the Raleigh Fitkin Memorial Pavilion for children, to cost $600,000, is nearly ready for occupancy.

With this progress in building construction have gone developments which will enable the Yale School of Medicine better to take its part in forwarding the plan for studying man from the mental, social and physical point of view. The work in psychiatry and mental hygiene has been strengthened both by additions of personnel in the fundamental sciences underlying study of the mind, and by adding representatives of the various aspects of treatment and prevention of mental disease. A number of appointments were made following Dr. Winternitz's visit last spring to the European clinics for the study of the central nervous system.

Not only is the mind to be taken into consideration by the Yale School of Medicine in dealing with all its problems of health, but economic and social factors as well, the study of these latter factors to be furthered by cooperation, through the Institute of Human Relations, with the university divisions of the social sciences. Furthermore, the School of Medicine is to devote its attention not alone to the sick in mind and body, but also to the well, with the aim of keeping people from getting sick. The report says:

> Not only is the negative side of well-being important, but the positive must also be understood. Furthermore, no one is so efficient that further assistance in one or another direction might not be advantageous. Too long has

all energy, almost, been directed toward the totally weak; too long have the strong been unaided—indeed, without consideration.

That the two great fields, the physical and the psychic, can not be sharply differentiated is obvious to every thinking person, especially, perhaps, to the physician who constantly is made conscious of the influence of physical illness on the psychic reaction of his patient. The more discerning recognize the contrary condition; they observe the influences of psychic disturbances, originating in social, economic and a vast number of other factors, upon the patient's physical reactions.

SCIENTIFIC NOTES AND NEWS

DR. JOHN DEWEY, professor of philosophy at Columbia University, will be the first William James lecturer at Harvard University on the foundation provided by the late Edgar Pierce, of the class of 1892, who at one time held a position in psychology at Harvard. Professor Dewey will give during the second half of the next academic year a series of ten public lectures and will conduct a seminar for graduate students in philosophy.

DR. RICHARD MILLS PEARCE, JR., general director of the division of medical education at the Rockefeller Foundation, formerly professor of medical research in the University of Pennsylvania, died suddenly on February 16, at the age of fifty-five years.

DR. WILLIAM ROYAL STOKES, bacteriologist of Baltimore, who handled some of the first infected parrots, has given his life in the battle with parrot fever, as did Harry B. Anderson, laboratory assistant at the U. S. Hygienic Laboratory. Dr. Daniel S. Hatfield, of Baltimore, and Dr. Charles Armstrong, of the U. S. Public Health Service, are both suffering from the disease, but their recovery is expected.

THE William H. Nichols Medal of the New York section of the American Chemical Society for 1930 has been awarded to Dr. Samuel Edward Sheppard, assistant director of the research department of the Eastman Kodak Company, Rochester. The award, bestowed for outstanding achievement in the chemistry of photography, will be presented to Dr. Sheppard at a joint meeting of the American Chemical Society, the Society of Chemical Industry, the Société de Chimie Industrielle and the American Electrochemical Society to be held on March 14 at the Chemists' Club, New York City. Dr. Sheppard will deliver an address on "The Reactions of Photographic Material to Light." Dr. C. E. K. Mees, director of research of the Eastman Kodak Company, will speak on "The Accomplishments of the Medalist," and Dr. Wilder D. Bancroft, of Cornell University, on "A Futuristic Study of the Medalist."

THE Lamme Medal of the American Institute of Electrical Engineers has been awarded to R. E. Hellmund, East Pittsburgh, Pennsylvania, "for his contributions to the design and development of rotating electrical machinery." It is expected that the medal will be presented at the summer convention of the institute, which is to be held in Toronto, Canada, from June 23 to 27.

THE Harrison Memorial Prize Selection Committee, consisting of the presidents of the Chemical Society, the Institute of Chemistry of Great Britain and Ireland, the Society of Chemical Industry and the Pharmaceutical Society has awarded the Harrison Memorial prize for 1929 to Dr. R. P. Linstead. The prize is given for conspicuously meritorious work in any branch of chemistry, pure or applied, and is to be regarded as an exceptional distinction to be conferred upon a chemist less than thirty years of age.

DR. O. E. JENNINGS, head of the department of botany of the University of Pittsburgh and curator of botany and director of education at the Carnegie Museum, received the honorary degree of doctor of science at the convocation of the University of Pittsburgh in February.

DR. P. S. SELWYN-CLARKE has received the decoration of the Médaille d'Argent des Epidémies from the French government for services in connection with the outbreak of yellow fever in West Africa.

DR. A. FUJINAMI, professor of pathology in the Kyoto Imperial University, became a member of the Imperial Japanese Academy of Science at a recent general meeting of the academy.

DR. DONALD F. JONES, geneticist, of the Connecticut Agricultural Experiment Station at New Haven, has been named chairman of the publications committee of the Sixth International Genetics Congress to be held at Cornell University in 1933.

DR. O. L. SPONSLER, professor of botany in the University of California at Los Angeles, has been chosen president of the Western Society of Naturalists.

AS has been reported in SCIENCE, the meeting of the British Association this year will be held in Bristol, from September 3 to 10, when Sir Thomas Holland will be succeeded in the presidential chair by Professor F. O. Bower. The newly elected presidents of the sections are announced in *Nature* to be as follows: A—Mathematical and Physical Sciences, Dr. F. E. Smith; B—Chemistry, Professor G. T. Morgan; C—Geology, Professor O. T. Jones; D—Zoology, Dr.

W. T. Calman; E—Geography, Professor P. M. Roxby; F—Economics, Professor T. E. Gregory; G—Engineering, Sir Ernest Moir; H—Anthropology, Dr. H. S. Harrison; I—Physiology, Professor H. S. Raper; J—Psychology, Professor C. W. Valentine; K—Botany, Dr. A. W. Hill; L—Education, Right Honorable Lord Eustace Percy, P.C.; M—Agriculture, Dr. P. J. du Toit.

PROFESSOR ARTHUR E. KENNELLY, since 1902 professor of electrical engineering at Harvard University, retires at the close of the academic year with the title of professor emeritus.

DR. WILLIAM B. CASTLE has been promoted to be assistant professor of medicine in the Harvard Medical School, not assistant professor of physiology as was stated in a recent number of SCIENCE.

THOMAS D. BURLEIGH, of Georgia, for the last nine years head of the division of forestry of the Georgia State College of Agriculture and previous to that time a technical field assistant in the U. S. Forest Service, has been appointed associate biologist in the Bureau of Biological Survey to carry on research work on the relation of wild life to forests authorized by the recently enacted McSweeney-McNary Forestry Research Act. He will be stationed at the Appalachian Forest Experiment Station at Asheville, North Carolina.

DR. E. P. CLARK, research chemist of the Interstate Cottonseed Crushers' Association, and Dr. Herbert L. J. Haller, associate in the department of chemistry of the Rockefeller Institute for Medical Research, have accepted appointment in the insecticide division of the chemical and technological research unit of the Bureau of Chemistry and Soils.

ASSOCIATE STATE FORESTER WILLIS M. BAKER, of the New Jersey Department of Conservation, has been appointed director of the newly created Pennsylvania Forest Research Institute at Mont Alto. This is the first institute of its kind maintained by a state department of forestry in the United States.

ASSOCIATE DEAN COLLINS P. BLISS has been named dean of the college of engineering of New York University to succeed Dean Charles H. Snow, the retiring administrative head of the college.

DR. SHIRLEY W. WYNNE, New York City health commissioner, has been appointed professor of preventive medicine at the New York Polyclinic Medical School and Hospital.

THE Royal Society has awarded a Foulerton research studentship to Dr. A. S. Parkes, who at present holds a Beit research fellowship. Dr. Parkes is working in the department of physiology and biochemistry at University College, London.

DR. J. G. DUSSER DE BARENNE, of the University of Utrecht, has been appointed professor of physiology at Yale University. Dr. Eugen Kahn, of the University of Munich, has been appointed professor of psychiatry.

K. Y. WANG, who graduated in agriculture at Purdue University in 1928 and obtained his master's degree in August, 1928, has been appointed president of Kiangai Agricultural College in Nanchang, Kiangai, China.

THE following appointments, recently made by the British secretary of state for the colonies, are recorded in *Nature:* Mr. A. G. G. Hill, botanist, Nigeria, to be plant breeding officer (senior geneticist), Mauritius; Mr. N. Humphrey, to be agricultural officer, Kenya; Mr. L. R. Doughty, to be geneticist, East African Agricultural Research Station, Amani, Tanganyika Territory; Mr. J. P. Mead, personal assistant to the conservator of forests, Malaya, to be director of forestry, Malaya; Mr. J. N. Oliphant, conservator of forests, British Honduras, to be deputy director of forestry, Malaya.

DR. D. L. EDSALL, dean of the Harvard Medical School and dean of the School of Public Health, has a leave of absence from May 1 to commencement and for the first half of 1930–31. He will use his sabbatical leave in travel and research abroad. During his absence, Dr. C. K. Drinker has been appointed acting dean of the School of Public Health, and Dr. Worth Hale will serve as acting dean of the Medical School.

DR. KONSTANTIN ECONOMO, professor of psychiatry and neurology in Vienna, has been invited to lecture at Columbia University on his researches on the brain.

DR. L. W. NORDHEIM, of the University of Göttingen, has been appointed visiting professor of theoretical physics at the Ohio State University for the spring quarter. He will lecture on recent developments in wave mechanics.

PROFESSOR ERNEST L. SCOTT, of the department of physiology of the College of Physicians and Surgeons, Columbia University, spent December and January at the School of Tropical Medicine at San Juan, Porto Rico. On January 14 he gave an address before the faculty and students on "Blood Sugar." Colonel Edward B. Vedder, U. S. Army School, arrived on January 20. Colonel Vedder was a visiting lecturer this year. He gave four lectures before the faculty and students.

PROFESSOR BRISTOW ADAMS, of the office of publication of the New York State College of Agriculture, who has leave of absence, left at the end of the term to spend the equivalent term at the University of

Hawaii, Honolulu, to give two courses there, one on the conservation of natural resources and the other on journalistic writing. From Hawaii, which he will leave about the end of May, Professor Adams will visit Japan, China and the Philippines.

DR. GEORGE D. HUBBARD, head of the department of geology and geography in Oberlin College, will spend the coming college year in travel and study in Europe. His plans include visits to most of the countries of Europe and to Egypt, Syria and Palestine.

PROFESSOR C. T. BRUES, of Harvard University, delivered a series of two lectures on "Insects and Human Welfare" at Coker College on January 27 and 28.

PROFESSOR CHARLES J. CHAMBERLAIN, of the botanical department of the University of Chicago, will conduct a lecture course at the University of California at Los Angeles during the coming spring semester.

DR. E. P. WIGHTMAN, a member of the Research Laboratory of the Eastman Kodak Company, gave an illustrated popular lecture on "How, Why and What is a Photograph" on February 13 before the Brooklyn Institute of Arts and Sciences. The lecture included motion pictures, showing the development of a photographic negative, and motion-picture photomicrographs illustrating the action of various photographic developers on individual grains of silver helide.

DR. H. H. LOVE, of Cornell University, gave the first series of lectures under the Frank Azor Spragg Memorial Fund from January 27 to 31 at the Michigan State College. The fund is in honor of Professor F. A. Spragg, who was one of the first men employed by an agricultural experiment station to devote his entire time to plant breeding. He began his work with the Michigan Experiment Station in 1906, and continued with that institution until his death in 1924. The most prominent varieties developed by him are: Hardigan alfalfa, Robust beans, Michigan Black Barbless barley, Wolverine and Worthy oats, Rosen rye and Red Rock wheat.

THE seventy-ninth meeting of the American Chemical Society will be held in Atlanta, Georgia, from April 7 to 11. A public address to the citizens of Atlanta entitled "Chemistry Comes South" will be made by Dr. Harrison E. Howe. The division of industrial and engineering chemistry has arranged a series of papers dealing with industrial and chemical processes as practiced in the south. The division of physical and inorganic chemistry has arranged a symposium on analytical chemistry which includes the following speakers and subjects: G. L. Clark, "X-Ray Analysis"; C. C. Nitchie, "Spectrographic Analysis"; C. W. Mason, "Microscopic Methods"; V. K. LaMer, "Modern Theories of Solutions"; D. A. MacInnes, "Potentiometric Methods for H-Ion Concentration"; H. B. Weiser, "Colloid Chemistry in Analytical Chemistry."

THE American Institute of New York held its annual election on February 13, after a long contest concerning the policies of the institute. The progressive ticket won over the conservative by a majority of fifty-five votes, and the following officers were elected: *President,* Mr. A. Cressy Morrison, of the Union Carbide Company; *Vice-president,* Dr. Otis W. Caldwell, director of the institute of school experimentation, Columbia University; *Secretary,* Dr. William Crocker, director of the Boyce Thompson Institute for Plant Research; *Treasurer,* Alfred Knight, vice-president of Standard Brands, Inc.; *Trustees,* Dr. Oscar Riddle, Dr. H. H. Sheldon, Dr. Charles H. Herty, Walter Kutzleb, Fred M. Becker, Maurice Holland, Justus Ruperti, H. T. Newcomb, Willis H. Carrier.

THE United States Civil Service Commission announces examinations for physicist, $3,800 a year; associate physicist, $3,200 a year and assistant physicist, $2,600 a year in the Bureau of Standards. Applications for these positions must be on file with the Civil Service Commission, Washington, D. C., not later than August 14.

THE museum of the American Indian, Heye Foundation, has asked the Government of Denmark, through the State Department, for permission to send an expedition to Scoresby Sound and its vicinity in East Greenland to collect ethnological material for a study of the coast Eskimo of that region, as well as archeological specimens. Should permission be granted the expedition will be led by Captain Robert A. Bartlett, who during thirty-three years has made more than forty voyages to the Arctic.

DISCUSSION

ETHER STRUCTURE

STUDIES in recent years of the dielectric properties of gases and electrolytes show that electrical forces and inductions in such media depend upon the polarized ions or "dipoles" of the medium. Why not extend the same concepts to electrical forces and inductions across a vacuum, that is, through the ether? We must then conceive of the ether as a medium with

a structure, that is, with "ether dipoles." By such a concept, we would obtain an explanation of "electric forces acting at a distance," something that has been very vague, or lacking, since the discard of the Maxwell ether displacement theory of electric charges and electric forces. The discovery of the electron disproved the ether displacement theory of electric charges, but it did not remove (for many physicists) the need of the ether concept in explaining electric waves, whether luminous or non-luminous. The actual structure of the ether will be a speculative problem until experiments have given us more facts in ether physics; but in view of the above, we can think of the ether as having an indefinitely large number of infinitesimal "ether dipoles." It is possible to think of these ether dipoles as ether concentration points, or some other hypothesis of their constitution may be assumed. Further, it may well be a question whether the ether is a continuous or a discontinuous medium. Indeed the quantum theory suggests the possibility of a discontinuous ether.

The "dipole ether hypothesis," while thus suggested by dielectric phenomena, should explain other phenomena if it is to gain acceptance. It seems to give a plausible explanation of the puzzle of how the electron can be at the same time a particle and a wave. A free electron, due to its electrical field, is surrounded by directed ether dipoles, an envelop of ether dipoles. The waves can be thought of as due to vibrations in this envelop of ether dipoles. The vibrations in the dipole envelop would arise from motions and changes of motions of the electron, these vibrations being transmitted to the surrounding ether atmosphere as waves. A free proton in motion, and possibly a moving atom, would also show wave characteristics for similar reasons.

We should also expect every material body to be surrounded by an atmosphere of ether dipoles, an atmosphere characteristic of the particular kind of matter. This follows of course from the fact that each kind of matter has its distinctive numbers of electrons and protons, and hence its field. We may find in these distinctive ether enveloping atmospheres possible explanations of various boundary electrical phenomena, such as electromotive forces of contact, electrosmosis, etc.

From the above, it is natural to ask about the possibilities of detecting ether structure by methods using X-rays or other short waves. The "grain" of ether structure may be too fine for ordinary X-ray methods, but there is always a possibility of some method giving results, and positive results would be important indeed for ether physics.

ALBERT P. CARMAN

JANUARY 8, 1930

AUDITORY NERVE IMPULSES

BY placing an electrode on the cat's auditory nerve near the medulla, with a grounded electrode elsewhere on the body, and leading the action currents through an amplifier to a telephone receiver, the writers have found that sound stimuli applied to the ear of the animal are reproduced in the receiver with great fidelity. Speech is easily understandable. Simple tones, as from tuning forks, are received at frequencies which, so far as the observer can determine by ear, are identical with the original. Frequencies as high as 3,300 cycles per second are audible.

Numerous checks have been used to guard against the possibility of artifact. No response was obtained when the active electrode was placed on any other tissue. After destruction by pithing of the cochlea on the electrode side, the intensity of the response was diminished; after destruction of the cochlea on the other side as well, the response ceased. However, the possibility is still conceivable that these results are due to purely mechanical action of the nerve, which is brought about by mechanical vibrations transmitted from the cochlear structure acting as a special receptor and transmitter.

Further experiments are in progress.

ERNEST GLEN WEVER
CHARLES W. BRAY

PSYCHOLOGICAL LABORATORY,
PRINCETON UNIVERSITY

SCIENTIFIC NAMES

THE problem of scientific names, and it is no small one, has been brought to attention once more by Professor James G. Needham, of Cornell University,[1] some twenty years after an earlier presentation along similar lines. He certainly can not be accused of being precipitate or of lacking patience. His main plea in both cases is for something more workable. This latter communication was evidently stimulated by an extremely long and to our mind presumably distinctive generic combination, namely, *Brachyuropushkydermatogammarus,* and another nearly equally long name. The author of this generic term evidently failed to live up to possibilities or it might have been prefixed by any one of several very popular combinations such as *eu-, pseud-* or *acanth-*, the last permitting thirty-six letters in the name, and this could be increased considerably by combinations of two or three prefixes. In other words, there is no limit to this sort of multiplication, and if our little crayfish had several related genera, it would easily be possible to bring together an unrivaled assemblage of generic names which under present conditions would be handed down and presumably used by countless generations, provided writer's cramp did not incapacitate

[1] SCIENCE, 71: 26–28, 1930.

the scientists of the entire human race. The trouble with these extremely long generic terms is that they tell too much or take too long to tell it, just whichever way one pleases to regard it. If we could only take the *push* out of the generic name given above and devote the energy expressed by this four-lettered expressive combination to working out a more usable and really a more scientific nomenclature, the effort would not be in vain.

There are something over 160,000 zoological genera, and they are increasing at the rate of 1500 names a year, and most zoologists are firmly possessed of the idea that they are building up a permanent nomenclature. It is reasonable to believe that this manufacturing of generic names will continue for another series of years, possibly a hundred or more, and with this expected great increase in generic names having twenty-six to thirty or even more letters each, one may reasonably ask the question as to whether zoological nomenclature is likely to be the permanent structure so many desire. It requires no great mentality to believe that an exceedingly complex and increasingly cumbrous system is bound to break by its own weight and the pity of it all is that this is entirely unnecessary if we will but face the realities and carefully study the possibilities of systematic terms. The chemist discovered this years ago. Zoologists are certainly headed in that direction. There is no need of following the present nomenclatural highway until it disappears in a limitless morass.

Professor Needham suggests a backward step in the use of many of the older generic names in the earlier and broader interpretation. That may be preferable in some respects to the enormously multiplied genera we find in some groups to-day. It is, however, only a palliative, and we cannot expect to stem the taxonomic tide which would define more closely the systematic relations of the multitudinous forms grouped under zoology, to say nothing of those coming within the domain of botany. It is very desirable that the names in these two great groups should not be duplicated and that their formulation and application should be governed by the same general principles. Professor Needham also suggests enlisting the services of a psychologist in solving this problem. It is more than probable that the knowledge of an expert librarian would assist greatly. It certainly looks as though aid would be really needed from some source, and the writer is by no means certain that a very considerable service might not be obtained from the psychiatrist, since our system of zoological nomenclature, as it stands to-day, with all due respect to the unselfishness and conscientiousness of earlier workers, cannot by any stretch of the imagination be considered on the whole especially creditable to the intelligence and the acumen of scientists. One only needs to scrutinize a considerable list of generic names, 50,000 are better than 10,000, to arrive at the conclusion that there is much in formation and the assignment of these names which could be greatly bettered and the change would mean an immense increase in the efficiency of all subsequent scientific workers. Just a moment's consideration is sufficient to show that the use of thousands of several popular prefixes to differentiate between generic names can hardly be sustained on the basis that the prefix selected is really definitive. The present-day tendency is so strongly toward differentiation, partly forced by an indefensibly illogical, unwieldy system, that many generic names have little really definitive, and in many cases are to all intents and purposes meaningless combinations of letters, except as they arbitrarily express a taxonomic concept.

The system of Linnaeus was admirable for his day and generation and served its purpose for a long series of years. It does not follow, however, that a system devised then is equally satisfactory now any more than we would be willing to revert to sail, the canal-boat, and the ox-cart for transportation.

The scientific nomenclature of both animals and plants is, in our opinion, a matter of very great importance and one worthy of extended study not necessarily by men who have distinguished themselves as taxonomists, since the ability to interpret generic and specific differences does not necessarily imply an equal grasp of the complex problems involved in building up a workable or permanent or nearly permanent nomenclature. There should be some way devised, possibly through the good offices of the National Research Council, of bringing together a representative group of scientists for an exhaustive study of the problems of nomenclature. It is doubtful if the International Commission on Zoological Nomenclature would prove the best agency to handle such a general problem, though it has done much toward stabilizing nomenclatural practices. The project should be on a broad basis, biological in scope. The writer has in mind a fundamental study of the possibilities of using generic terms, letter combinations, or something of the kind in the development of a nearly automatic classifying system. There are great possibilities in some such arrangement. Anyone of several would greatly improve the heterogeneous combinations produced by the thousands to meet supposedly scientific necessities. Zoologists and botanists, biologists, if one prefers, are faced with a tremendous problem in classification and one destined to increase enormously as various groups are studied more closely. It is inescapable. The problem is akin to that of the librarian in charge of

one of the largest libraries in the world. Experience has shown the latter that thirty-letter or thirty-numeral combinations are not necessary to the precise location of the million or more volumes in their charge. The same is true in regard to our hosts of generic names.

Primarily nomenclature is an aid to differentiation, the placing or locating of species. Generic names are not intellectual memorials of their originators. They are really tools for the correct placing of the multitude of interesting forms inhabiting the earth. The rule of priority was invoked in the interest of stability. Developments show gross carelessness in creating new names. We must face the facts and rise to the opportunity, not wait until overwhelmed by insurmountable obstacles. Does man exist for science or is science an aid to human understanding? It should be possible to secure a dispassionate, constructive survey of the entire nomenclatural situation by a body, not only capable of interpretation but one with sufficient influence eventually to bring into effect measures which will at least better existing conditions

The studies of the writer (he has been dealing with great numbers of generic names in the past thirty years) have shown that four-letter combinations are sufficient to throw every generic name in zoology into its family, or at least into a group of small families. They are not euphonious. The possibilities of prefix and numeral combinations may well be studied and while it may not be feasible to displace some of the lovely, descriptive generic names which have been foisted upon practically defenseless workers, there might be devised a placing system which would make possible arrangement either alphabetically or by numerical series and the placing of all biological genera in family or near family groups. This alone would mean a tremendous saving in bibliographic and taxonomic work and under such conditions we might be able to view with calmness the proposal of a new generic name with 30 or 40 or more letters.

E. PORTER FELT

BARTLETT TREE RESEARCH LABORATORIES,
STAMFORD, CONNECTICUT

WITH reference to Dr. Needham's discussion,[1] I venture to offer a few comments on particular points. He must be well aware that the Dybowski polysyllabics, one of which he quotes, have been declared invalid by the International Commission. But the Lake Baikal gammarids have valid generic names, and if Dr. Needham had seen them, as I have, he would no more want to refer them all to *Gammarus* than he would wish to return to a Linnaean or Fabrician nomenclature for the genera of dragon-flies. The five names of bumblebees, as he is also well aware, are not the names of five species, as such, but are the designations of certain mutations of subspecies of those species. The generic names quoted are names of subgenera or sections of *Bombus,* which a few authors have employed in a generic sense. This practice is not likely to become general. In the Palearctic region, the species of *Bombus* present various races or subspecies, which are designated by trinomials. These races include various mutants, for which quadrinomials are employed. The nomenclature simply follows the complexity of nature. Now here is the dilemma. If we use only binomials we are either obliged greatly to increase the number of specific names, and lose sight of the grouping of the races under aggregate species; or we must simply ignore the races in our system, as in fact Dr. Needham does in his book on dragon-flies. Similarly, if we use only trinomials, and give races and mutations equal standing in nomenclature, we again introduce confusion and obscure the facts of nature. But the system of trinomials and quadrinomials actually serves to conserve the binomial system of Linnaeus, leaving the names of the aggregate species as they were, and ready to be employed by all those who are not interested in the refinements of modern taxonomy.

There is a valid objection to the excessive multiplication of generic names, but the several cases must be considered by experts, and not decided offhand by those who have not studied the animals concerned.

There is a serious objection to excessively long generic names. Long ago it was proposed that no generic name should contain more than six syllables. I have in my own practice treated this as a rule and it ought to be made one, permitting of no exceptions.

Much trouble has resulted from the practice of "emending" names in the past, but the rules permit the correction of a name if a slip of the pen or a misprint can be declared to have occurred. In recent years, particularly, the ignorance of some authors has led to the publication of misspelled or ungrammatical names, and it should be permissible to correct these, at least if the change is made without undue delay.

But after all, the troubles due to vagaries of nomenclature are nothing compared with those due to careless and inaccurate scientific work, or even to the inevitable fallibility of human powers of perception and judgment.

T. D. A. COCKERELL

UNIVERSITY OF COLORADO

WE are interested in the discussion of scientific names by Professor Needham.[1] It is inevitable that

[1] SCIENCE, 71: 26, January 10, 1930.

[1] SCIENCE, 71: 26, January 10, 1930.

the little crustacean, laboriously designated *Brachyuropushkydermatogammarus,* is destined to be unhonored and unsung, unless by chance it be given a convenient and satisfactory nickname. It is unfortunate to launch a species upon the scientific sea with the handicap of a "bad name" which can never be lived down or in any way altered.

There is a well-known law of evolution which operates toward the extinction of gigantic and cumbersome forms: the dinosaurs are already gone, the elephants and redwoods are much reduced in distribution, the whales are doomed, and likewise, we hope, battleships! Nature acts as though she appreciated the unwarranted expense involved in producing a huge beast, the difficulty of its establishing, the extravagance of its maintenance and finally its susceptibility to a single fatal accident which results in the destruction of tons of material.

A natural selection working in taxonomy should and doubtless will eliminate the long technical names which we frequently see, because they are not only useless to most of us but they impede the work of those who are expected to profit from their existence.

EDWARD L. TROXELL

TRINITY COLLEGE,
HARTFORD, CONNECTICUT

SOIL SCIENCE

IN his note in the January 17, 1930, issue of SCIENCE relative to the term "pedology" as name for soil science, Dr. W. A. Hamor called attention to its use since 1896 as the name for another science, as recognized by lexicographers.

Webster gives this: "pedology; paedology . . . 1: Med. Pediatrics; 2. Child study." In contrast Webster gives this for geology, for example, which is recognized by lexicographers as a science: "The science which treats of the history of the earth and its life. . . ." The Encyclopaedia Britannica (fourteenth edition, 1929) gives this: "Children, diseases of. The study of this subject, known also as pediatrics, is comparatively modern."

Probably most soils scientists know that in 1862 Fallou (German) published a 488-page book on pedology. Fallou defined natural soil science or pedology as the description of the nature of soils, no matter what their relation to vegetation or industry might be.

W. W. WEIR

NEW YORK CITY

DR. HAMOR'S objection to the adoption of "pedology" as a name for soil science[1] seems to be sound and his suggestion of "humology" for that use is excellent.

As a worker in the field of soil science I enter my protest against such a monstrosity as "chthonology," in which, I think, the public would join. Scientific nomenclature is cluttered up too much with useless letters; why not apply the principles of simplified spelling? If we must have Greek, write it "thonology," even at the risk of its being interpreted "wolf science" by some Greek scholar. But *humology* seems better.

A. M. PETER

EXPERIMENT STATION,
LEXINGTON, KY.

PRESS NOTICE OF THE DES MOINES MEETING IN IOWA

MR. T. J. EDMONDS, who had charge of the local publicity for the Des Moines meeting, has prepared a statement showing the amount of space given the meeting by the press of Iowa.

In advance of the meeting there were published 1,710 articles (including 30 editorials and 36 photographs) totaling 10,998 column inches. In addition, advance notice of the meeting was given from 15 radio stations.

During the meeting there were published 7,436 articles (including 33 editorials and 88 photographs) totaling 74,781 column inches.

The press of the State of Iowa alone therefore published in connection with the meeting 9,146 articles (including 63 editorials and 124 photographs) totaling 85,779 column inches.

In the Des Moines papers the meeting was noticed by 15 eight-column headlines.

AUSTIN H. CLARK,
Director, Press Service

REPORTS

AWARDS OF THE AMERICAN SOCIETY OF PLANT PHYSIOLOGISTS

THE CHARLES REID BARNES LIFE MEMBERSHIP

AT the recent Des Moines meeting of the American Society of Plant Physiologists, the fourth and fifth awards of the Charles Reid Barnes life membership were made. The two recipients of the honor were Professor George J. Peirce and Professor Charles A. Shull. Accounts of the establishment of this honorary life membership and of the first two awards are found in previous issues of SCIENCE[1] Each year the

[1] SCIENCE, 71: 70.
[1] SCIENCE, 65: 175–176, 1927; 67: 264–265, 1928.

society awards to some deserving member an honorary life membership in honor of Charles Reid Barnes, the first professor of plant physiology at the University of Chicago. The three previous awards, in the order of their occurrence, were to Burton E. Livingston, Francis E. Lloyd and Herman A. Spoehr.

Professor Peirce took his B. S. degree at Harvard University in 1890 and his Ph.D. degree at the University of Leipzig in 1894. From 1895 to 1897 he was assistant professor of botany at Indiana University. Since 1897 he has been at Stanford University, first as assistant professor of botany, then as associate professor of plant physiology and since 1910 as professor of botany. From 1909 to 1910 he was a collaborator with the U. S. Forest Service, and from 1910 to 1911 he was a special agent of the U. S. Department of Justice in the study of the effects of smoke on vegetation.

Professor Peirce has contributed numerous scientific papers, especially in the fields of respiration, irritability, parasitism and the effects of smoke and other atmospheric impurities on vegetation. In 1903 he published his text-book of plant physiology—the first book on the subject to be published in this country. In 1925, he published "The Physiology of Plants and the Principles of Food Production." He is also one of the authors of a text-book of biology.

Professor Peirce is no less known as a teacher than as an investigator and author. He takes a very great personal interest in all his students and has the happy faculty of arousing and holding their interest in the subject being discussed. He is a man of kindly personality, of great human sympathy and of broad interests. His interests extend beyond the realm of his science, as is evidenced by his election to the chairmanship of the Palo Alto Board of Health and the Palo Alto Chapter of the Red Cross.

Professor Shull received his B. S. at the University of Chicago in 1905 and his Ph.D. degree in 1915. During part of his graduate student days he was a member of Dr. Barnes's classes and also his student assistant. He traces much of his interest in plant physiology to the inspiration received from Dr. Barnes. He was assistant professor of biology at Transylvania College from 1906 to 1908, and professor of biology and geology from 1908 to 1912. From 1912 to 1918 he was at the University of Kansas, first as assistant professor of botany and then as associate professor of plant physiology and genetics. During the period from 1918 to 1921, he was professor of plant physiology and head of the department of botany at the University of Kentucky. He went to the University of Chicago in 1921 as associate professor of plant physiology. Since 1925 he has been professor of plant physiology at this institution.

Professor Shull has exerted a great influence on plant physiology in several different ways, notably as an investigator, as a teacher and as an editor. He is the author of a large number of scientific papers which have added much to our knowledge, especially in the fields of seed germination, permeability, water relations of seeds and plants, water-holding power of soils, respiration and the energy relations of the leaf. His teaching is characterized by clarity of presentation and by contagious enthusiasm. His lectures fairly bristle with the problems remaining to be solved in plant physiology. Many a student has been led to make plant physiology his field of research through listening to his lectures. The recent successful establishment of the American Society of Plant Physiologists and its official journal, *Plant Physiology*, and the placing of these enterprises on a firm foundation were due more to the courage, clear vision and hard work of Professor Shull than to any other one person. He is editor-in-chief of *Plant Physiology* and associate editor of *Botanical Gazette*.

The members of the committee for the fourth and fifth awards of the Charles Reid Barnes life membership are J. D. Sayre, Earl S. Johnston, R. B. Dustman, J. H. Gourley and Walter Thomas, *chairman*.

THE STEPHEN HALES PRIZE

IN 1927, Professor Charles A. Shull, then president of the American Society of Plant Physiologists, conceived the idea that this year, the two hundred and fiftieth anniversary of the birth of Stephen Hales, would be an opportune time for the society to do honor to this pioneer plant physiologist and biological investigator who stands as a prominent landmark in the scientific horizon of the seventeenth and eighteenth centuries. Under Professor Shull's leadership, there was established by the society the Stephen Hales prize endowment fund. Every other year a prize and certificate are to be awarded to some deserving plant physiologist of North America. The first award was made at the recent Des Moines meeting.

After long and careful consideration, the committee made the award to Professor D. R. Hoagland, professor of plant nutrition at the University of California and chemist of the California Agricultural Experiment Station. Professor Hoagland took his A. B. degree at Stanford University in 1907 and his A. M. degree at the University of Wisconsin in 1913. He was instructor in agricultural chemistry at the University of California from 1907 to 1910, chemist in the U. S. Department of Agriculture from 1910 to 1913 and assistant professor of agricultural chemistry at the University of California from 1913 to 1920. Since 1920 he has been associate professor of plant

nutrition at this institution, and since 1925 he has been chemist in the California Agricultural Experiment Station.

Professor Hoagland has contributed numerous scientific papers especially in the fields of mineral nutrition of plants, soil acidity and soil and plant interrelationships. In the field of the mineral nutrition of plants he has done much to elucidate the process of the absorption of salts by the plant and to explain some of the anomalies appearing in this important plant physiological process. In studying the mineral nutrition of plants, it is of course important to control the environment. Professor Hoagland has devised an apparatus by which the conditions of light, temperature, humidity and culture solution may be controlled to such a degree that the growth of plants in duplicate experiments is identical as measured by yield, number of tillers, height of tops and other external features.

In view of the very great services of Professor Hoagland to plant physiology, the committee is very glad to make the first award of the Stephen Hales prize to him. The members of the committee are A. L. Bakke, C. R. Ball and J. B. Overton, *chairman*.

S. V. EATON,
President
H. R. KRAYBILL,
Secretary-treasurer

SCIENTIFIC APPARATUS AND LABORATORY METHODS

THE COLORIMETRIC DETERMINATION OF SOIL REACTION

WHILE electrometric methods are now generally applied in the determination of the reaction of soil suspensions, the different electrodes do not always agree in their indications, and for this and other reasons it may be desired to obtain results by a colorimetric method. In order to observe slight differences in tint of indicators, it is essential that a clear water extract of the soil be obtained. Various means to this end have been employed, such as filtration on conical or Buchner funnel with paper repeatedly extracted until the apparent reaction of pure water is not affected by contact with it, dialysis, centrifuging, etc. Some of these procedures are objectionable on account of the time required, others because it is difficult to protect the extract from contamination. It has been found that clear water extracts can be obtained from many soils without the use of any filter medium other than the soil itself, in a reasonable length of time and without great risk of contamination, by means of very simple apparatus. The method may not succeed with soils which tend to run together and lose their crumb structure when wetted, or with samples that have been ground, but in any case it is easy to determine the possibility of obtaining a clear extract in this way.

A percolating tube is made from a thin Pyrex test-tube, about 17 mm internal diameter, by drawing out the lower end to a cone about 5 cm long and 2 mm wide at the narrowest point. It is cut at this point and the thin tip heated to thicken and contract to about 1 mm inside diameter at the tip. About 25 g of the finely granular air dry or slightly moist soil is charged into this percolator, gently shaken down and about the same amount of water poured on top. With most soils the water will penetrate the soil without much difficulty, and after a few drops have run from the tip, the percolate will be clear. The clear extract is received in the test-tube which is to be used for the color comparison, marked at 10 ml with a wax pencil, and suitably supported. The conical bottom of the percolator fits into the mouth of this test-tube and excludes air. As soon as sufficient extract has been collected, the indicator is added and the color compared with buffer mixtures in similar tubes. As water extracts of most soils are practically unbuffered, it is essential for accuracy to use isohydric indicator solutions and other precautions described by Fawcett and Acree[1] in a recent paper. Several duplicate samples of soil should be percolated at the same time in order to have sufficient extract for the repeated tests which may be necessary to determine the reaction with precision.

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A METHOD FOR DETERMINING HARDINESS IN PLANTS

METHODS of measuring the hardiness of plants by correlation with chemical and physical properties have been the subject of great interest and investigation in the past few years. A new method based upon the degree of exosmosis of electrolytes from tissue after freezing has been worked out in a preliminary way, and determinations of exosmosis have been made at weekly intervals throughout the autumn on alfalfa roots from varieties of known hardiness. The amount of outward diffusion into distilled water is determined by conductivity measurements, which indicate a pro-

[1] Edna H. Fawcett and S. F. Acree, "The Problem of Dilution in Colorimetric H-ion Measurements. 1. Isohydric Indicator Methods for Accurate Determination of pH in Very Dilute Solutions," *Journal of Bacteriology*, vol. 17, no. 3: 163–204.

gressive development of hardiness throughout the autumn period in the hardy varieties, while the tender varieties show no such change. At the end of November, the differences in exosmosis as measured by this method amount to several hundred per cent. between the very tender and the most hardy varieties. Colorimetric tests for chlorides and nitrates in the exudate from the frozen roots correlate very well with the conductivity measurements, and regeneration of growth in the greenhouse indicates that injury by freezing may be closely estimated by this method. The author plans to investigate its application to other plants and to perfect details of technique. The precise experimental method and results will be published soon.

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A SOURCE OF DIASTASE

EXPERIMENTS designed to illustrate digestion are not uncommon in beginning courses in botany, the action of diastase or amylase on starch paste being a common method of procedure. Ptyalin, contained in the saliva, may be successfully employed, but for classes involving large numbers of students this is difficult to obtain in sufficient quantity. Malt diastase may be prepared, if facilities are available, but the extraction must be performed with care, and the yield is small unless large-scale operations are employed. The ordinary commercial preparations, available on the market under various trade names, are unsuited, since they contain starch or sugar or both and thus vitiate the results before the experiments are started. After a considerable canvass of the situation, a source of diastase has been found which meets the requirements of a starch- and sugar-free enzym which is not only entirely suitable for experiments of the nature indicated, but which is useful in a number of physiological and biological procedures as well. Since it seems apparent that teachers generally are unaware of an easily obtainable enzym, this note may not be out of place.

The Digestive Ferments Company manufactures and sells a digestive ferment under the trade name of "Pangestin." The manufacturers claim that it is capable of converting eighty parts of potato starch into water soluble substances in five minutes in accordance with the U. S. P. X Revision test for pancreatin. Pangestin has good solubility, with a small amount of extraneous material not identified. Both the dry enzym and aqueous solution give a negative test for starch. Freshly made aqueous solution and the same after standing fifteen hours give a negative test for glucose. The digestive power has been tried on potato, corn and other starches in 2 per cent. aqueous paste form. Ten minutes at room temperature, and without an activator, give very positive tests for sugar with Fehling's solution. Cornstarch is apparently acted upon more rapidly than potato starch.

Pangestin is not a simple enzym. Not only is it strongly amylolytic, but, because it is of animal origin, it is also proteolytic, as shown by its power to digest completely the white of egg, and lipolytic, as shown by the hydrolysis of oils, such as cottonseed and olive. It is therefore a mixture of enzyms. This does not, however, detract from its usefulness, but rather enhances it. So far as the writer has been able to ascertain, Pangestin is the only trade product made in America conforming to the requirements of a starch- and sugar-free enzym, although several other chemical companies have signified their interest in the production of a purified product. One German product submitted has also been tested out, but has been found unsatisfactory.

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SPECIAL ARTICLES

NITROGEN FIXATION BY BLUE-GREEN ALGAE

NITROGEN fixation, a property common to several species of bacteria and a few fungi, has long been suspected as being a property of algae also. Considerable research by several investigators with green algae (*Chlorophyceae*) has failed to demonstrate that any one of these possesses the ability to use free nitrogen gas. The few claims that have been made to the contrary have been disproved, to the satisfaction of probably all the workers in this field. The writers have recently had occasion to check this point further. Three cultures of green algae, namely, *Chlamydomonas, Chlorella* and *Scenedesmus,* obtained from Cornell University through the courtesy of Dr. F. B. Wann, and a *Pleurococcus,* isolated from a local soil, were repeatedly tested for nitrogen-fixing powers, with negative results.

What are the facts regarding the nitrogen-fixing ability of the other common group of algae, known as the blue-greens (*Cyanophyceae*)? Many articles have appeared during the past forty years in which references are made to the fact that under various

experimental conditions, particularly in greenhouse pots, there occurred gains in nitrogen or increased growth of higher plants where abundant growths of blue-green algae were present. In these greenhouse experiments the blue-green algae were growing in association not only with higher plants in most cases but with bacteria, fungi, green algae and all the other common soil micro-organisms. Hence it was impossible to attribute the fixation directly to the blue-greens. The fact that these forms were nearly always present in abundant numbers where the unexpected nitrogen fixations occurred naturally led many workers to suspect that they were the agents responsible. Others, however, stressed the fact that Azotobacter and other free-living bacteria were nearly always found closely associated with the dense algal growths. This observation formed a basis for the other commonly accepted idea that a sort of symbiotic relationship exists between the two types of organisms. In such a symbiosis the algae by means of their chlorophyll are supposed to furnish carbohydrates for the non-symbiotic bacteria, while the latter fix nitrogen, a portion of which the algae use. During the past few years this symbiotic theory has probably been more generally accepted than that of direct fixation by algae, because no one could demonstrate that any alga could use free nitrogen. In the case of blue-greens the failure to demonstrate fixation may be attributed largely to the difficulty of securing pure cultures. In the gelatinous algal sheaths, bacteria thrive and are eliminated only with great difficulty by ordinary bacteriological methods.

A few years ago the writers began nitrogen-fixation studies with blue-green algae, isolated from a local soil. In preliminary culture work these organisms in mixture with other soil organisms were found to fix as high as 40 mg nitrogen per 100 cc of nutrient solution, containing 1 per cent. sucrose, when exposed to sunlight in the laboratory window for approximately a year. These crude cultures were partially purified by being repeatedly plated out. In order to make the final separation, the algal cultures containing bacteria were exposed to ultra-violet light from a quartz mercury vapor lamp for periods varying from one to twenty minutes. Most of the bacteria were killed by exposures longer than five minutes, but several attempts had to be made before complete sterilization resulted without appreciable injury to the algae. In January, 1929, pure cultures were finally secured by use of this method. Freedom from bacteria and other organisms was determined by inoculation onto four commonly used bacteriological media, as well as by repeated direct microscopic examinations. When grown on a nitrogen-free medium through which a stream of air, purified by passing through sulphuric acid and sterile water, was constantly bubbled, the organisms were demonstrated to possess the ability to fix atmospheric nitrogen. During a period of 75 days' incubation, an average fixation of 5 mg nitrogen was obtained per 100 cc of a culture medium originally containing no nitrogen or sugar. Where one gram of sucrose was added the fixation was 8.5 mg nitrogen. The cultures were grown at room temperature in a southwest window where they received direct sunlight for approximately three hours each day. The growth requirements for the blue-green algae used are very simple; a complete nutrient medium usually produces the best growth, but even tap water, containing .05 per cent. K_2HPO_4, with a satisfactory light source produces a rather good growth. In the absence of light, if sugar is present in the medium, the organisms live for months and retain their normal color, but the growth is very slight. In the dark, fixation was so slight as to be almost within experimental error, but growth even in the presence of combined nitrogen was also slight. Additional experiments to determine this point are in progress.

The identification of the alga used in most of the experiments has not as yet been definitely made. Professor W. R. Taylor, of the University of Pennsylvania, is giving a very generous portion of his time in helping out on this phase of the work. He reports that because of the fact that it has grown for such a long time on artificial media, it is in many respects rather abnormal. Considerable additional culture work will be necessary to identify definitely the organism but, in general, Professor Taylor states that its characteristics agree rather closely with those of *Anabaena variabilis* Kütz. Another culture corresponds in most respects to *Phormidium molle* (Kütz) Gom. These names are given here merely as provisional designations until further studies are completed. The point of immediate importance is not the exact identification of the nitrogen-fixing species but rather the fact that a pure culture of a blue-green can use free nitrogen gas.

It is not the intention of the writers to leave the impression that this is the first time that blue-green algae have been grown in pure culture or demonstrated to use free nitrogen. Pringsheim[1] in 1912 was probably the first to grow blue-green algae, *Oscillatoria* and *Nostoc* species, free of bacteria. He did not demonstrate that either could fix nitrogen. A short time after the writers had obtained *A. variabilis* Kütz in pure culture and while a series of experiments was already under way to test its ability to use free nitro-

[1] E. G. Pringsheim, "Kulturversuche mit chlorophyllführenden Mikroorganismen. Zur Physiologie der Schizophyceen," *Beitr. Z. Biol. d. Pflanzen*, Bd. 12, 1913: 49–108.

gen, the recent excellent paper of K. Drewes[2] came to our attention. He grew *A. variabilis, Nostoc punctiforme* and another species of *Anabaena* in pure culture and obtained about 2 to 3 mg of nitrogen fixed per 250 cc of nitrogen-free nutrient solution during a period of two months. It is quite a coincidence that workers so far separated should have isolated what appears to be the same organism from different soils and independently demonstrated its powers of nitrogen fixation at so nearly the same time. The results agree very closely except that the quantities of nitrogen fixed were relatively larger in our results, 1.67 mg nitrogen per 100 cc per month in a sugar-free solution as contrasted with a corresponding figure of 0.6 mg nitrogen in Drewes' results. This point is not important, however, since the light intensity, quantity of CO_2 available, temperature and amount of inoculum are all important factors affecting the total fixation.

The economic significance of these findings remains to be determined, and work is being continued in this laboratory in the hope of learning more about the factors that influence the fixation, importance in nature and mechanism of the process. The fact, now definitely proved, that a chlorophyll-containing plant growing in pure culture can fix nitrogen in quantities far greater than experimental error puts additional emphasis upon the old question as to whether higher green plants in pure culture can use free nitrogen. Undoubtedly, more nitrogen is now being fixed in nature than can be accounted for by any previously known agencies of fixation. This statement is based largely upon the large energy requirements of non-symbiotic nitrogen-fixing organisms. The blue-green algae get most of their energy from sunlight, and the nitrogen that they fix costs little. We know that many species of these organisms are found in almost all soils and in fresh water, but their importance is largely unknown. It is, at least, significant that Robbins[3] in his study of the algae of Colorado soils, where nitrogenous salts often accumulate to such an extent as to cause "niter spots," isolated twenty-one species of algae, all but two of which were blue-greens. The most prevalent organisms were *Phormidium, Nostoc, Anabaena, Nodularia* and *Stigonema*. Robbins did not, of course, know that many or perhaps all of these forms are able to fix nitrogen. He considered that they are of importance in nitrogen fixation because they furnish energy for Azotobacter.

In the light of these results it is likely that blue-green algae are important non-symbiotic nitrogen-fixing organisms. They may, indeed, be the most important nitrogen-fixing agents in many agricultural soils.

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OCTOBER 9, 1929

[2] K. Drewes, "Über die Assimilation des Luftstickstoffs durch Blaualgen," *Centbl. Bakt., Par.*, etc., 2. Abt., Bd. 76 (1928): 88–101.

[3] W. W. Robbins, "Algae in Some Colorado Soils," Colorado Agr. Exp. Sta. Bull. 184 (1912): 24–36.

SALINE DRINKING WATER

BECAUSE of the ease of modern modes of transportation, man of to-day is exposed to an ever-increasing change in living conditions to which he must adjust himself. Without a doubt, water is the greatest variable which one encounters in passing from one region to another, and it is this variable which is responsible for the greatest physiological changes in the body.

The waters of certain areas of the United States are either quite free from dissolved salt or contain only very small amounts. Such waters are called "soft" waters. In contrast to these practically salt-free waters are those waters which have come into contact with limestone beds and have taken up calcium carbonate in the form of the bicarbonate. Such waters are said to have temporary hardness. Surface waters which have passed through alkaline soils, or well and spring waters which have come into contact with salt beds, will dissolve these salts and will consequently have a brackish taste.

Large sections of the southwestern part of the United States have alkaline soils which impregnate surface waters with salts, and in addition there are in these regions extensive salt beds which contaminate the deep wells and many of the springs. The salt content of the rivers in these sections is often so high that it will kill vegetation along their shores. The salts present in these waters are sodium chloride, calcium chloride, calcium sulphate and magnesium sulphate. Analyses of waters from such sections have shown these salts in certain instances to be present in excess of 200,000 p.p.m., and 5,000 to 50,000 p.p.m. are common.

The question of the possible deleterious effects of such waters upon the animal organism immediately presents itself. Travelers drink such salty waters with considerable hesitation on account of their unpleasant taste and disagreeable physiologic action. Live-stock growers frequently attribute the death or poor condition of their stock to the salty waters which the animals must drink. This is especially true where the cattle drink from small streams which receive waste saline waters from the oil wells.

Strange as it may seem, scientific literature does not answer this vital question. There are volumes of semi-popular articles dealing with the medicinal properties of certain spring water. In the pharmaceutical litera-

ture the physiological action of certain salts is recorded, but practically nothing can be found in regard to the physiological effects of the ingestion of saline drinking water. In a pamphlet published in 1925 by the U. S. Public Health Service, "Drinking Water Standards," an arbitrary standard is set for waters which are to be used on interstate commerce carriers, but it is a matter of common knowledge that much of the water being used continually by people in the southwest contains more saline matter than is permitted by the U. S. Health Service, and, nevertheless, there is no evidence of any deleterious effects arising as a result.

An investigation was begun in an attempt to answer the following questions in regard to saline waters. 1. What is the effect of the presence of considerable quantities of salt in our daily drinking water? 2. At what percentage does salt become deleterious to the organism? 3. Is it possible for the organism to adjust itself in time to saline drinking waters? The experimental animals used in this work were rats and other small animals. They were placed upon well-balanced rations supplemented with drinking water which was prepared by dissolving different amounts of various salts in distilled water. Sodium chloride was added to the water in amounts ranging from 500 p.p.m. to 50,000 p.p.m. Similar series were tried using calcium chloride, magnesium sulphate, sodium carbonate, sodium bicarbonate and calcium sulphate (up to the limits of saturation). The general appearance of the animals, their growth rate, their ability to reproduce and to rear their young and the condition of their organs at death were considered in studying the effects of the various salts upon the animal.

The first point determined was the maximum percentage of a single salt which could be administered in the water without producing harmful effects upon the animal. Mixtures of the salts were also added to the water to determine whether there might possibly be an antagonistic action between the various ions: that is, whether the deleterious effect of one ion might not be offset by the effect of a second ion. Such an antagonistic ion effect has been observed in certain body processes and also in plant nutrient solutions between sodium, calcium and magnesium ions.

From the data obtained, it is possible to postulate that 15,000 p.p.m. of magnesium sulphate has a retarding effect upon growth and that increases the mortality rate of the young. Twenty-five thousand p.p.m. of magnesium sulphate have a decided toxic effect. Calcium chloride produces even more harmful effects. No normal litters were found among rats drinking 10,000 p.p.m. of this salt; 15,000 p.p.m. lessen the growth rate, and 25,000 p.p.m. produce death. Ten thousand p.p.m. of sodium chloride affect reproduction unfavorably; 20,000 p.p.m. cause the growth of the animal to be stunted, and 25,000 p.p.m. produce a rapid decline in weight. Sodium carbonate is decidedly deleterious at a concentration of 20,000 p.p.m., and reproduction was interfered with at quite low levels. Sodium hydrogen carbonate is less injurious to the animals. A combination of 20,000 p.p.m. of sodium chloride and 5,000 p.p.m. of magnesium sulphate or calcium chloride inhibits growth. The results of feeding various other combinations of salts seem to indicate that in animals there is no antagonistic action between the ions such as there is in plants. The chlorine ion appears to be more toxic than either the carbonate or the sulphate ion. The calcium and magnesium ions are more harmful than the sodium ion. This can be explained by certain known physiological actions of ions. It is interesting to note that as the salt concentration increased the amount of water consumed by the animal daily also increased up to the point where the animal preferred to die of thirst rather than drink the necessary amount.

When animals which had been given a salty water were placed on distilled water soon after their first loss in weight, they rapidly regained their weight and appeared not to have been permanently injured by the saline content of the water. Animals which have received the salty water for some time seem better able to stand such water than do animals which are suddenly changed from distilled water to salty water.

The above results indicate that the continued use of saline solutions as drinking water produces injurious effects upon the animal organism. The concentration at which the solution becomes harmful varies with the particular salt used. The animal organism can apparently accustom itself within limits to a saline water, but in all probability it can not become adjusted to the effects of high concentrations.

V. G. HELLER
C. H. LARWOOD

OKLAHOMA AGRICULTURAL AND MECHANICAL COLLEGE

BOOKS RECEIVED

CHAMBERLIN, THOMAS C., and ROLLIN D. SALISBURY. *College Text-Book of Geology.* Part II. Historical Geology. Rewritten and Revised by Rollin T. Chamberlin and Paul MacClintock. Pp. ix + 497 + xxviii. 353 figs. Holt. $3.75.

MACPHERSON, HECTOR. *Modern Cosmologies.* Pp. viii + 131. 12 illustrations. Oxford University Press. $2.75.

SANDFORD, K. S., and W. J. AAKELL. *Paleolithic Man and The Nile-Faiyum Divide.* Pp. xv + 77. 11 plates. 25 text figures. University of Chicago Press. $5.00.

TERRILL, H. M., and C. T. ULREY. *X-Ray Technology.* Pp. viii + 256. 143 figs. Van Nostrand. $4.50.

WALTHER, JOHANNES. *Leopoldine.* Pp. xviii + 375. In German and English editions. Verlag Von Quelle & Meyer. Leipzig, Germany.